The Yale Journal of Biology and Medicine

The HERBERT THOMS *Number*

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The HERBERT THOMS *Number*

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Volume 25 Number 6

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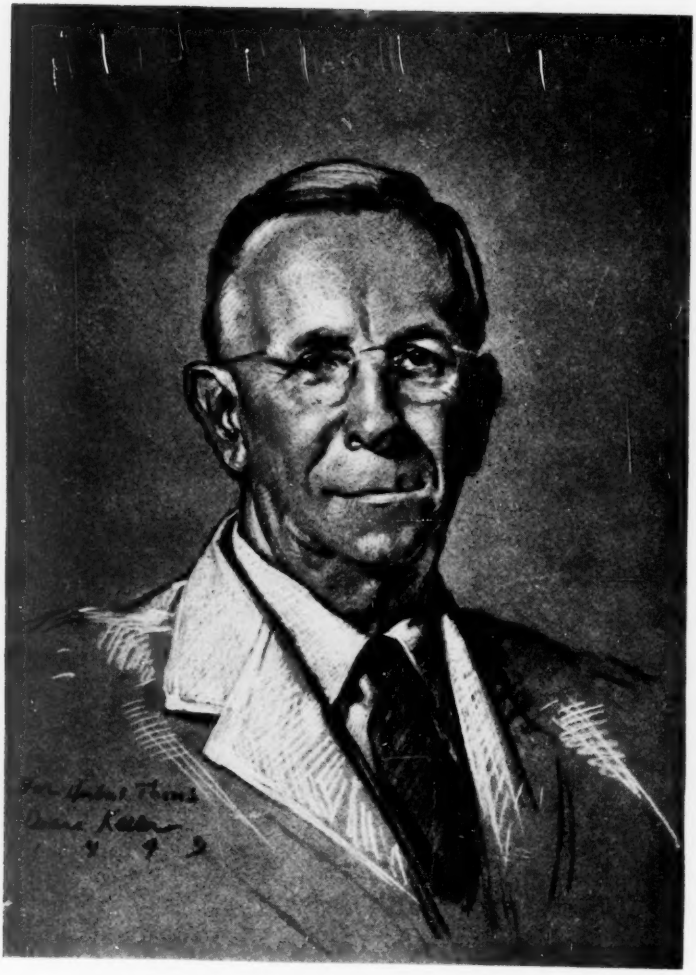

HERBERT THOMS

(From a crayon portrait by Deane Keller)

HERBERT KING THOMS

The Progress of a Scotsman*

SAMUEL C. HARVEY**

It is a great pleasure to have the opportunity of making a contribution to this volume published in honor of "Herb" Thoms. Others will consider various activities and responsibilities not primarily concerned with his career as a teacher; this essay will attempt to portray him as a Yale medical student of the class of 1910, to describe the ensuing years of preparation in his special field, and the consummation of his education as a member of the faculty.

Medical education in the United States was throughout the years 1906 to 1910 in a transitional phase, and the Department of Medicine of Yale University was no exception. In 1904 the Council on Medical Education was established by the American Medical Association, and a detailed study of some 160 medical colleges then in existence was initiated. In 1909 the Carnegie Foundation for the Advancement of Teaching engaged Abraham Flexner to make a study of the facts already accumulated and a personal inspection of the schools with a critical evaluation of each. His explosive report made in 1910 blew the situation wide open to public view, bringing about a marked decrease in the number of medical schools as well as an increase in public support of those remaining, which led to remarkable improvement in all aspects of medical education. A few schools were at the time already in the process of "modernization," as Flexner put it. Harvard had increased its curriculum from three years to four in 1892, having previously raised its entrance requirements. In 1893 the Johns Hopkins Medical School opened with a four-year curriculum and a bachelor's degree the requirement for admission. In 1896 Yale extended its course from three to four years; it had required for some time previously entrance qualifications corresponding to those of the College. These were raised in 1909 to a minimum of two years of collegiate study.

Flexner in his report was critical also of the caliber of the faculties, of the laboratory facilities, and of the libraries. However, he cited the Yale School as one of three—the others being Harvard and the Washington University at St. Louis—which were actually in the process of improvement at the time of inspection. Here he directed his criticism for the most part at the lack of support, financial and otherwise, from the University, and the tenuous connection with the New Haven Hospital, where the

* From the Department of the History of Medicine, Yale University School of Medicine.

** William H. Carmalt Professor of Surgery Emeritus.

School had but limited control over clinical facilities for teaching. In the subsequent decade the foundation was laid for the correction of these deficiencies. This was the situation when Thoms entered the Medical School in 1906.

He was born in Waterbury on January 6, 1885, as a result of which he retires at the age of sixty-eight on June 30, 1953; with his usual prescience he postponed his appearance until after the New Year else his retirement would have been in 1952. However, his lack of foresight as regards his place of birth was soon corrected by moving to Torrington, undoubtedly with the view to becoming a distinguished son of Litchfield County.

His preparation for entry into the School of Medicine was obtained in the Vermont Academy and the Waterbury High School. He was one of 61 entering the Medical School of whom only five had bachelor's degrees. Of the latter, two were from Yale College, two from the Sheffield Scientific School, and one from Brown. Some twelve taking a "combined course" went elsewhere at the end of the first year. At the beginning of the second year, in 1907, 27 of those entering were left but five in addition were repeating from the previous class (1909), making a total of 32. In 1908, 14 remained of the original class, to which were added 21 from the preceding class and from elsewhere, bringing the total up to 35. At that time there were six with collegiate degrees, but at the end of this year one, a newcomer, had disappeared and another voluntarily dropped out to return to graduate in 1911. At the beginning of the fourth year there were 28 in the class, 14 of these being from the original entering group, and at the end of the academic year in 1910 this number graduated.

Over these four years there had been 46 men temporarily members of the class. This scholastic mortality was not peculiar to the class of 1910. In 1911, 53 were admitted, of which 20 were graduated, 14 having lasted throughout the four years; some 40 others were in the class at one time or another. The class of 1912 included 57 students in 1908; 29 were graduated, 33 having been lost in transit, but the class of 1913 numbered only 22 in 1909, the year that the standards of admission were raised to a minimum of two years of collegiate study. Nine were graduated, 8 being from the original class. The change in policy led to a low rate of admission which was not materially improved until the end of World War I. This, of course, very seriously affected the financial status of the School, for up to this time the funds available were almost entirely derived from tuition fees, the University contributing relatively little. It was only when material endowment was obtained and when the University assumed a degree of financial responsibility for the School that the situation improved.

It is doubtful if the students, including Thoms, were cognizant of the fundamental changes that were underway during the period from 1906 to 1910. They were all well aware, however, of the heavy mortality at the end of each year. For those who were serious-minded, had had some experience

in studying, and were endowed with an average innate intelligence, the means were at hand for enlightenment. It was true then, as it is today in many schools, that a considerable part of the teaching was by lectures and recitations. There were, however, adequate laboratories available in the building on York Street for the first two years and a major part of one's time was spent there. The faculty, while sparse in number, included excellent teachers. Professors Ferris in anatomy, Henderson in physiology, and Bartlett in pathology were each in his way quite effective. That which was lacking was an atmosphere of research, a circumstance which was by no means unique in schools of medicine at that time.

In the clinical years, however, there was more of this and as Thoms entered this phase of his training he found Professor George Blumer in medicine, a disciple of Osler who had come to Yale in 1906, and as his assistant, newly arrived from Harvard and the Massachusetts General Hospital, Wilder Tileston. Professor William H. Carmalt, who had been a mainstay of the School for over 35 years, retired from the chair of surgery in 1906 and this position was filled in 1907 by Joseph M. Flint, a distinguished anatomist who had worked with Mall at the Johns Hopkins and had returned directly to Yale from two years' study of surgery in Germany. Obstetrics and gynecology, which Thoms was later to take as his own field of endeavor, was in the very capable hands of Ramsey and his assistant, Rand, they having come from Kelly's clinic at the Johns Hopkins in the early 1900's.

The major contact with patients in these and the other specialties was in the New Haven Dispensary which some years previously had been erected on land across from the Hospital. Under the instigation of Carmalt, Thatcher, and Dean Smith of the Medical School, this had been purchased by the University with a view eventually to moving the Medical School there. The facilities in this institution were excellent and the clinical material ample, but in the Hospital the situation was not satisfactory. It was not until 1913 that an agreement was entered into between it and the University by means of which "patients in the general and semi-private wards" could be used "for purposes of instruction, investigation and research . . . so far as is consistent with their treatment and welfare." This contract also specified that the chiefs of the medical, surgical, pediatric, and obstetrical and gynecological services should give their full time to their university responsibilities. While this was not consummated until some six years later, the World War I intervening, Blumer and Flint were for all practical purposes giving full time to teaching. Although they did not have complete control of all these patients, they had a sufficient number to enable them to transfer their teaching from the clinical amphitheater to the wards.

Again however, as in the first two years, funds sufficient for the adequate support of these two departments were not at hand, with the result that the staffs were restricted in number, and investigation, beyond that of

routine, was limited. But clinical laboratories were established during Thoms' third year by the remodeling of a house adjacent to the Dispensary and the adding to it of a new building providing a surgical classroom and an experimental laboratory with animal facilities in the basement. This was in effect the Hunterian Laboratory at the Johns Hopkins transplanted to New Haven, and for this development Flint deserved great credit. It represented the introduction of experimental clinical medicine at Yale and soon served to provide an atmosphere most stimulating to the students, and very notably for Thoms.

Previous to this, his performance was something better than the average of his class; this can be safely stated, for the majority of his fellow students failed and he did not. What his ambition may have been other than that of graduating is no more apparent than it is in most students. Curiously enough, two of the class of 1910 are recorded in the "Year Book" as expecting to enter the Medical Corps of the Army and make it their life work and one of these is Thoms. Neither did, however, perhaps because they were not encouraged to do so—to the degree that they would be today.

Moreover, it is not clear what his motivation may have been in the first place for aspiring to be a physician and for coming to the Department of Medicine of Yale University; in any case the decision was somewhat delayed for he was 21 years of age on entering. To have taken a collegiate course preparatory to doing this might not only have taxed the financial resources at his disposal but would have delayed him. It is of interest that the mean entrance age of his class was between 21 and 22 and that this did not differ materially from those having collegiate preparation. Presumably influential in the decision was his older brother, who graduated at the head of the class of 1894 of Yale College. He had by 1906 established himself as an able lawyer and an energetic leader of the Democratic party in Waterbury. His encouragement and assistance may well have been a determining factor leading to Thoms' becoming a student in another learned profession, that of medicine and at Yale.

The annals of the medical student do not vary greatly in respect to time or place. Living conditions were much the same in 1906 to 1910 as they are today, save that Thoms did not have to find accommodations for a wife and children. He lived, as did most of the medical students, by migration from place to place. At first he was in the dormitory of the old Divinity School, next in what was said to have been an attic in one of the small houses on High Street, and finally in a similar secluded retreat at 52 Park Street which had been taken over by a group of eleven medical students. He differed from his confreres in that he survived with minimum expenditures, while maintaining, nevertheless, a wide range of interests.

The first year he was a member of the freshman glee club and in the subsequent three years, of the college choir; an early trend toward journalism was evident in his being a member of the advisory board of the *Yale*

Daily News and the editorial board of the *Yale Medical Journal,* of which he was chairman in his senior year. It was noted by one of his classmates that he was the most versatile and active man in the class. In the "class vote" he took first place as being the one most likely to succeed, although he did not even place among such categories as "grinds," and "hard workers," nor was he noted for the attributes of "brilliance" and "studiousness." Thoms was, however, far out ahead among the "grafters," in recognition, it may be presumed, of his Scottish ancestry. These citations are amusing even if puerile and would suggest that here was a fellow who was interested in many things from which he got much satisfaction somewhat enhanced by a shrewd economy.

Among these interests, and prophetic of his future activities, was his work in the new experimental laboratory on Cedar Street under the stimulation of Flint. The latter was at that time greatly interested in thoracic surgery. The Meltzer-Auer method of intubating the trachea of the experimental animal had been recently described and this made it possible to open the chest and carry on surgical procedures within it. Thoms' graduating thesis was entitled "Rectal and Pharyngeal Anesthesia, with Demonstration of New Apparatus and a Report of Cases," and was an initial step in the giving of intratracheal anesthesia. For this contribution he split the Keese Prize with his classmate Johnson who significantly enough wrote on "An Experimental Study on Heart Valves and their Sounds." Thoms promptly published his thesis in the *Yale Medical Journal* of May 10, 1910, it being his first contribution to medical literature.

While this undoubtedly gave him great satisfaction, it apparently did not assist in the solution of his immediate problem, that of obtaining a hospital appointment. There were not many good internships available at that time and the most sought after were those of the "Big Four" in New York where the competition was severe, the Rhode Island General, which customarily took two men from Yale each year, and the New Haven Hospital, whose quota was five. In all, eight of the 1910 graduates were placed in these, the remainder being scattered for the most part in Connecticut. Thoms received an appointment at the Backus Hospital in Norwich but transferred to the Memorial in New London, Connecticut, apparently with a view to entering general practice. There he interned for one year and then served in 1912 as an assistant resident in the Sloane Hospital for Women in New York. In the interim between this and his next move he acquired a charming wife, a daughter, and a delightful old stone house, as well as a lucrative practice. At the same time he also published three papers. In 1914 he decided that he would specialize in the field of obstetrics, so he pulled up his stakes and went to the Johns Hopkins, where he worked under J. Whitridge Williams, the Professor of Obstetrics, for one year.

Meanwhile Otto Ramsey, the Professor of Obstetrics at Yale, had died, and J. Morris Slemons, then at the University of California, who had been

for many years a junior associate of Williams, was called to succeed him. This undoubtedly seemed to Thoms a propitious circumstance and he therefore returned to New Haven to establish himself in practice and to work under Slemons as a "laboratory assistant . . . eight hours a day" with the salary of a technician. At this time began his association with Arthur Morse, whom Slemons had brought with him as an assistant professor.

In 1917 two world-shaking incidents occurred, the one our entry into World War I, and the other, the reorganization of the Department of Pathology on the accession of a new chairman, Professor M. C. Winternitz. Shortly Slemons left for Europe, and Pathology became involved in a laboratory training program for medical officers. In the turmoil Thoms, who had become an assistant in pathology for 1916-17, was, for the time being, disassociated from any formal connection with the School. In this period from 1915 to 1918, he published eight papers, all in the field of obstetrics, the first and most important being the first of many on pelvimetry.

From this time until 1927 Thoms had a growing and active practice in obstetrics, but this was not allowed to interfere with his productiveness, as is shown by ten publications. Of these, three are in "Roentgen Pelvimetry" laying the grounds for his most important contribution to the field of obstetrics. During this same time he wrote five papers having to do with the history of medicine, thus entering upon an avocation which will now stand him in good stead. What is more significant, perhaps, were his efforts to broaden his education—in compensation, one may presume, for his lack of a collegiate experience—by taking up the study of the German language and, in Yale College, of English literature. Through the latter he acquired an acquaintance that subsequently led to his becoming a member of the Elizabethan Club and an Associate Fellow of Timothy Dwight College. In 1920 he was one of the founders of the Beaumont Club and gave the first paper presented before it. He was also, somewhat later, one of the instigators of the students' Nathan Smith Club. In his spare time, when he could find nothing else to do, he developed a talent for etching and oil painting, particularly in relation to the sea and ships, of which he had become enamored in his New London days.

Meanwhile, the organization and physical assets of the School of Medicine had undergone a great transformation under the enthusiasm and drive of Winternitz who had taken over the responsibility of the Deanship in 1920. At this time Slemons resigned to enter practice in Los Angeles, and Morse was placed at the head of the Department of Obstetrics, a position he was to hold until 1947 when he reached the age of retirement and became Professor Emeritus. In 1925 Thoms, who had now abundantly demonstrated his qualifications for it, was made an Associate Clinical Professor of Obstetrics and Gynecology, and in 1927, in order to be free from dependency upon practice, he elected to become a member of the "full-time

staff" as Associate Professor. He had at last reached the goal for which he had essentially and perhaps fortuitously prepared himself. In 1947 he became Professor and Chairman of the Department of Obstetrics and Gynecology.

The twenty-five years of his academic career have amply demonstrated the wisdom of this decision both for himself and for the Medical School. He has carried his weight in the routine teaching and care of patients as well as in the innumerable committees of the faculty, many of which have had to do with policy. More than any other member of the faculty, perhaps, he has been influential in bringing, by example, to the attention of the students the importance of a humanistic approach to medicine. Most important he has during this time continued to make important contributions in his field.

The studies in pelvimetry as shown by the titles in Thoms' bibliography have been continued throughout this time. In the more recent years he has played an important part in conjunction with the Department of Pediatrics in the "rooming-in" experiment. In this country his publications on "natural child birth" have been unique and have served to stir up as much controversy among the obstetricians as did Oliver Wendell Holmes in his essay on "puerperal fever."

Important, likewise, have been studies carried out in his infertility clinic under the auspices and with the support of the League for Planned Parenthood. This and his activities in the medical societies of the state and in the Clinical Congress, and as literary editor of the *Connecticut State Medical Journal,* when taken together, have been of great value in reducing the tendency towards isolation of the School in its relations with the physicians and laity.

It is often stated that retiring from a vocation gives one an opportunity to do those things which he has always wanted to do. "Herb" Thoms, it would seem, has always done the things that he has wanted to do, irrespective of obstacles, and is therefore fortunate in that his present recession only offers more time for his many avocations.

HERBERT THOMS

Extramural

CREIGHTON W. BARKER*

The eventful chapters of contemporaneous medical history in Connecticut have not yet received the historian's scrutiny and are still only records scattered here and there in organization archives and miscellaneous correspondence. Many of us have been a part of these events and as participants are poorly qualified to place values on them. As in all history, one must await the developing vista of time to bring out the enduring landmarks. A view of them now, however, does identify some of the persons whose presence on this historic way will be permanently noted, and of this small company Herbert Thoms will surely be a member.

Medical developments fall clearly into two areas. These are the scientific achievements, some laboratory and some clinical, and organizational progress which furthers and improves the relationship of physicians to each other and to their social environment.

Connecticut has been always fortunate in that its conspicuous figures in medical science have also most frequently been leaders in its medical societies, and Dr. Thoms has been outstanding among those who have played this dual rôle. Another contributor to this volume will consider his part in the scientific world, while here will be set down with warm appreciation the many splendid things he has done for his colleagues and their professional organizations.

It has long been customary in Connecticut for physicians to identify themselves with the medical societies in their communities and Dr. Thoms did this by becoming a member of the New Haven County Medical Association in 1915 soon after he took up his residence in New Haven. From the very beginning his spirit and interest were manifest in that Association and only two years after becoming a member he was elected its Clerk. The title Clerk in New Haven County is a pleasant survival of an old style; he is in fact the Secretary of the Association, but Herbert Thoms always insisted that he was the Clerk like his distinguished predecessors and nothing so commonplace as a Secretary. In those days the Clerk was elected for a term of five years and it was not until later, during the incumbency of Dr. Thoms' successor, that the term of office was changed from five years to one, in the belief that it was possible, perhaps with the changing times, to elect a person that the Association did not wish to retain in this important position for five years. This change was not suggested while Dr. Thoms was in office. During these years from 1917 to 1922 the Association

* Executive Secretary, Connecticut State Medical Society.

progressed and prospered even though it was perforce disrupted by the withdrawal of many of its members for service in World War I. As one looks back upon the machinery for recruiting medical officers at that time it appears that it may not have been as well organized as it was in a later war, but one is also impressed that what had to be done was accomplished with fairness and understanding. Also during these turbulent times many projects that were later realized were thought about and the Clerk was always in the midst of this future planning. It fell, fortunately, to the present writer to succeed Dr. Thoms as Clerk of the New Haven County Medical Association in 1922, when he became Vice-President. In April 1923 Thoms succeeded to the Presidency, so that during these two years following his Clerkship he carried forward many of the ideas that he had developed earlier. Even then it is likely that he was thinking of strengthening the structure of the county and state organizations and spreading their usefulness, so that more than a dozen years later, when the pattern of operation of the State Society was broadened, he was in the forefront of that development. His activities were by no means all directed toward the county and state organizations and while many things were going on at the county level, Thoms was not idle in the New Haven Medical Association. He had become interested in it through close association with John Lane, who, as Chairman of the Program Committee of the City Association, had done a great deal towards improving the quality of scientific material presented before the Association. Dr. Thoms succeeded Dr. Lane in this office and carried on in the high tradition that he had established. These two, with others, organized the now famous Beaumont Medical Club and, patterned after its success and in the belief that medical undergraduates could profit by knowledge of medical history, Thoms and other members of the medical faculty established the Nathan Smith Club, a student organization for the study of medicine's glorious past.

There is a strain of quiet humor in Herbert Thoms' life. It has seemed dormant for periods of time and then it unexpectedly buds; it probably came to its full flower in an escapade that might well be called "The Great Hoax." We well remember the day, sitting in the Thoms' library, when the affair was first suggested. It was too complicated for two conspirators to perpetrate and the Puckish help required was found by conversation with Lafayette Mendel, Milton Winternitz, and Frank Wheeler. The latter displayed some reluctance to join in the plot. It was really asking a good deal of him, but the ridiculousness of it appealed to him and he finally agreed.

Except for the omission of his part in it the best account of "The Great Hoax" was written at the time by Dr. Thoms himself and has never been published. With the addition of Thoms' part in it and emphasis upon the fact that the President of the Association had no previous idea of what it was all about, the story is now given here so that it will never be lost.

"Professor Stromberg's Visit to New Haven

On December 15, 1923, the medical profession of New Haven received the announcement for the next program of the City Medical Society and were surprised and delighted to learn that Professor Gustave Stromberg of the University of Upsala, Sweden, would address the meeting. The announcement also stated that Doctor Stromberg was Professor of Pediatrics and that he was coming to New Haven as the guest of Professor Lafayette Mendel, through whose efforts his first appearance in America was being made in our city.

The Secretary of the Medical Association, in honor of this event, arranged a dinner at the Graduates Club for Doctor Stromberg. This dinner was held on December 19th, the evening of the address. Forty members of the medical profession were present including a goodly representation from the Medical School. Several out-of-town guests were also there.

The Banquet Hall was appropriately decorated with the American and Swedish flags and the guests wore formal dress for the occasion. Unfortunately Doctor Mendel received word at the last moment that Professor Stromberg had missed his train in New York and would take a later train. The Professor therefore was unable to attend the dinner which was very much enjoyed by those present. Following this the ensemble adjourned to the Medical Society building. Here the largest crowd that ever attended a meeting packed the assembly hall to its capacity. Out-of-town guests included those from Milford, Meriden, and Hartford. The Hall was decorated similarly to the Banquet chamber with Swedish and American flags.

The first paper of the evening was presented by Dean Milton C. Winternitz of the Yale Medical School on "Diadokokinesis in Pathological Diagnosis." Dr. Winternitz presented in a masterly manner a very remarkable case with lantern slide illustrations. He also gave the autopsy findings in this case. It brought out in a very lucid manner the close relationship between the Glandulae Insularis Cervicalis and calcium metabolism. This and the fact that the patient had had fragilitas ossium as a child were some of the unusual features presented. Inasmuch as a number of prominent physicians in this city had seen this patient from time to time and had made several different diagnoses, the findings of the autopsy were listened to with great interest. Curiously these were in great contradistinction and in no way corresponded. This was made clear, however, by the speaker when he announced that through a laboratory error the autopsy had been performed on the wrong patient.

After the extraordinary applause had subsided Dr. Mendel received word that his guest had just arrived from the station in a taxicab. He was escorted by Dr. Mendel through the Assembly Hall, but it was impossible to secure a good view of our distinguished visitor because of his great fur coat which more or less submerged him. Professor Stromberg carried a small hand bag and was escorted to the rear room where he removed his overcoat.

Introductory remarks were made by Professor Mendel who drew attention to Professor Stromberg's prominence in the medical world; he also congratulated the New Haven Medical Association on their good fortune in having Professor Stromberg appear before them for his initial American appearance. At Dr. Mendel's suggestion, as a mark of respect and honor to the distinguished guest, the entire medical society stood while

he was being presented. Dr. Stromberg, a tall man with dark beard and hair, wore full dress. His bosom was adorned with a large red ribbon suggesting a foreign order and in his coat lapel the ribbon of the Legion of Honor was visible. He presented his credentials which were in the form of a large diploma from the Fellows of Upsala University who recommended their associate to the various medical societies in America.

The Professor made a short preliminary speech in which he explained that his lecture would be illustrated with lantern slides. This lecture, entitled "Some Pediatric Reminiscences" was accompanied by the slides which he explained he had collected from the Pathological Department of his University. He further explained that his talk had to do with a number of interesting children who had come under his care from time to time. The first picture showed a child of two years, dressed in an old fashioned frock which the Professor explained would probably be changed to a "Bloomer" if the picture were to be taken today. A subsequent slide showed a daguerreotype of a boy of nine years who later came to be known to us as Dr. William H. Carmalt. So on through the lecture the childhood pictures of the following physicians appeared on the screen: Drs. Swain, Cohane, Cheney, Bartlett, Steele, Rand, Sperry, Gompertz, and Townshend. After the prolonged applause which concluded this extraordinary lecture Dr. Stromberg was presented with a loving cup by the officers of the Medical Association for his faithful and earnest service. The cup, however, did not bear the name of Stromberg, but was engraved with that of the Treasurer of the Association, Dr. Frank H. Wheeler.

It took quite a little while for the atmosphere to clear after this extravaganza, and its promoters, when seen together, were long suspect. None the less Thoms and Winternitz soon had another idea, this one somewhat more sensible. It was to establish a formal, intensive, postgraduate teaching session for the State Medical Society that was to be called the Connecticut Clinical Congress. Like any good thing the Congress had its vicissitudes, and the august Council of the State Medical Society was not at first deeply impressed by the proposal. Perhaps it followed too closely after the Stromberg episode and there was a lack of confidence in its promoters. At one point it appeared that if it was to be done it would have to be an enterprise privately underwritten and managed by its originators, without the blessing of the State Society. Herbert Thoms was certain that it should be a part of the Society's activities and at about this time the American College of Surgeons was having a regional meeting in New Haven and the Congress was discussed with some of its members, notably Chester Brown. Finally after a period of indifference the proposal got to the Council at a meeting held on April 6, 1925. Many distinguished physicians appeared before the meeting and advocated the proposal and the discussion went far into the night. This was followed by an executive session, and the record of that meeting discloses that the Committee which had appeared before the Council were to be permitted to appoint spokesmen to speak before the several county associations and ascertain the reaction around the state. Finally, after what now seems to have been a peculiar and unnecessary

commotion, the House of Delegates voted on May 21, 1925 to accept the proposal for the fall postgraduate teaching congress. There was still some uncertainty as to just how the affair was to be financed, but that did not bother Herbert Thoms and he and his Clinical Congress were on the way. It has prospered for a quarter of a century and during those years he has had an important part in its planning and management.

Reference has already been made to Dr. Thoms' interest in medical history, but perhaps it would be more accurate to say the interest is in medical biography. He exhibits no unusual curiosity about history in general, but does have a deep regard for the people who have made medical history. It is difficult to go on an excursion into the lives of significant figures in American medicine and not find that Thoms has been there before you and knows as much about them as you will ever learn. His writings include many biographical sketches that are genuine contributions to American medical bibliography. Particularly important are his notes on the lives of the physician signers of the Declaration of Independence and his lengthy sketch on the life of James Gates Percival. The latter is probably the best story of the "Mad Genius of Kensington," one of America's most fantastic medical figures. It was this flair for writing that brought him to membership on the Advisory Board of *The Yale Journal of Biology and Medicine* when it was founded, and to the Literary Editorship of the *Connecticut State Medical Journal,* a position that he still holds. As such, he is responsible for the editorial policy of the State Medical Journal and many of his quiet comments on the medical scene roundabout us will long be remembered.

Two important events concerning the history of medicine in Connecticut have occurred in recent years. They were the 150th anniversary in 1934 of the founding of the New Haven County Medical Association and the Sesquicentennial of the State Medical Society in 1942. The New Haven County Association is one of the oldest extant medical societies in America, and its original records dating from January 5, 1784 are carefully cherished in the rare book vault of the New Haven Colony Historical Society, of which Dr. Thoms has been an officer for many years. As might have been expected, he played a significant part in these two never-to-be-forgotten milestones in our history. The Executive Committee of the New Haven County Medical Association made a proposal at the annual meeting of the Association on April 28, 1932 that an appropriate ceremony be planned for 1934, and a Committee, under the Chairmanship of George Blumer, was named to lay the preliminary plans for the celebration. Dr. Thoms was a member of that Committee. In October of 1932 the Sesquicentennial Committee reported to the Association and it was authorized to appoint special committees to develop an appropriate program for the meeting to be held on January 5, 1934, exactly 150 years to the day that the physicians of the county met in the Coffee House to form their Association. Dr. Thoms

A Drypoint by Dr. Thoms of the Entrance to the New Haven Hospital

became the Chairman of the Committee on Historical Meetings and Publications and it was this Committee that planned the historical program, which was held in the auditorium of the Yale Law School on that date. Dr. Thoms, with the late Dr. George H. Smith, directed the compilation and publication of transactions of the meeting in Volume 6, No. 3 of *The Yale Journal of Biology and Medicine,* January 1934. The outstanding feature of this volume, which was reprinted and widely distributed, was the facsimile reproduction of "Cases and Observations; by the Medical Society of New Haven County in the State of Connecticut," printed by J. Meigs in New Haven in 1788, and which has long been recognized as the first volume of medical transactions in America. Dr. Thoms also contributed an essay on "The Beginning of Medical Practice in New Haven Colony." It was at this meeting that the ancient privilege of the Society was revived and the degree of Doctor of Medicine *honoris causa* was conferred upon Russell H. Chittenden.

The Connecticut State Medical Society celebrated its 150th anniversary in 1942. The first meeting of the Society was held in Middletown on October 9, 1792 and the sesquicentennial celebration was held on June 3 and 4, 1952, at Wesleyan University in Middletown.

Dr. Thoms was Vice-Chairman of the Sesquicentennial Committee and had the leading part in the planning of that celebration and edited the now prized volume, *The Heritage of Connecticut Medicine.* The book, which was dedicated to James Douglas Gold, the grand old man of Connecticut medicine, was lovingly compiled by Dr. Thoms and contained nineteen monographs on many phases of the development of the medical profession in this state. Dr. Thoms himself contributed "Connecticut Doctors in Other Fields" and in it he presented little known information about a number of Connecticut physicians who were well known for accomplishments outside of medicine: Jared Eliot, minister of the gospel, agriculturalist, metallurgist; Jonathan Carver, famous eighteenth century traveler; Lyman Hall, native of Wallingford, signer of the Declaration of Independence for Georgia; Oliver Wolcott, signer and statesman; the "Connecticut Wits"—Elihu Hubbard Smith, Lemuel Hopkins, Mason Fitch Cogswell; and again he referred to the fascinating Percival. In his research for this contribution Dr. Thoms revived the memory of William Andrus Alcott, teacher and exponent of popular knowledge of anatomy and physiology and one of the founders of the American Physiological Society; and finally the story of that great early naturalist, Jared Potter Kirtland. It can be said without exaggeration that no other person, with the exception of Henry Bronson, has made so large a contribution to Connecticut medical biography.

It is not generally known that before medicine claimed Herbert Thoms' interest he had made an early resolve to be a professional photographer, and as a young man was seriously interested in photography and had a formal education in the photographic arts. Had he gone along with it, it is

easy to believe he would have made a conspicuous achievement. As it was it formed a background for his favorite avocation. Some people seem to be able to produce pictures in an off-hand way with the simplest equipment, while others, using all sorts of devices, never seem to accomplish much. Thoms is one of the former and some of his photographs are nothing short of remarkable. To go from photography to painting and drawing would seem like a natural transition, but it is not often encountered, and certainly requires a different kind of skill. Herbert Thoms did this and, although he has never entirely forsaken photography, for many years he has found an artistic outlet in making pictures. He has used many media—oil, water-color, and pastel—but he is probably best known for his dry points. He has tried etching also, but the dry points are the more successful and his prints have been exhibited in established showings in Connecticut and elsewhere. It would be extravagant for this artless friend to say that he is a great artist, but at least his talents are unusual. The subjects vary—ships at sea, buildings, birds, and vignettes of the countryside he loves so well. Sometimes his style is remindful of the bold strokes of Gordon Grant, while again, particularly in his prints of the Gothic buildings of Yale, one finds the delicate tracery of John Taylor Arms. Probably it is better to say that there is a Thoms style. His prints are widely sought by his friends and are included in many Connecticut collections.

A good deal could be added about Thoms as a fisherman whipping the stream alone in the early spring and as a sailor with his little cat-boat in the Thimble Islands. But most of all it should be said that he is a friend and good companion.

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112. The suboccipitobregmatic circumference. Am. J. Obst., 1940, *39*, 841-843 (with M. S. Godfried).
113. A study of the growth of the superior pelvic aperture in puberal girls. Anat. Rec., 1940, *76*, suppl. 2, 26 (with W. W. Greulich).
114. A discussion of roentgen pelvimetry and the description of a roentgen pelvimeter. Am. J. Roentg., 1940, *44*, 9-16.
115. Anesthésie à la reine. Am. J. Obst., 1940, *40*, 340-346.
116. Observations on the roentgenological evidence of fetal death. Surg. Gyn. Obst., 1940, *71*, 169-171.
117. Roentgen pelvimetry as a routine prenatal procedure. Am. J. Obst., 1940, *40*, 891-905.

1941

118. The Naegele pelvis. Am. J. Obst., 1941, *41*, 830-836.
119. Connecticut doctors in other fields. J. Connecticut M. Soc., 1941, *5*, 337-344.
120. The roentgenological survey of the pelvis. Yale J. Biol., 1941, *13*, 831-839 (with H. M. Wilson).
121. Theophilus Parvin, an important influence in American obstetrics and a pioneer in clinical teaching. Am. J. Obst., 1941, *42*, 346-351.
122. The clinical application of roentgen pelvimetry and a study of the results in 1,100 white women. Am. J. Obst., 1941, *42*, 957-975.
123. The new Yale Medical Library. J. Connecticut M. Soc., 1941, *5*, 689-691.

1942

124. Inversion of the uterus: a report of three cases and the use of chemotherapy. Yale J. Biol., 1942, *14*, 399-402.
125. Anesthesia and analgesia in obstetrics (with a report on the use of evipal soluble). J. Connecticut M. Soc., 1942, *6*, 327-332 (with H. C. Taylor).
126. *The heritage of Connecticut medicine.* H. Thoms, Ed. New Haven, Whaples-Bullis Company, 1942. Dr. Thoms contributed a chapter: Connecticut doctors in other fields, p. 209.
127. *The essentials of emergency treatment.* H. Thoms, Ed. Connecticut State Medical Society, 1942. 144 pp.
127a. The sesquicentennial celebration. J. Connecticut M. Soc., 1942, *6*, 26.

1943

128. The relation of the sacral promontory to the pelvic inlet. Am. J. Obst., 1943, *46*, 110-116.
129. Roentgen pelvimetry: a commentary. Surg. Gyn. Obst., 1943, *77*, 153-156.
130. Precision methods in cephalometry and pelvimetry. Am. J. Obst., 1943, *46*, 753-754.

1944

131. The obliquely ovate or Naegele pelvis. J. Am. M. Ass., 1944, *124*, 294-296.
132. A case of incarcerated uterus successfully treated in 1808. Am. J. Obst., 1944, *47*, 557-558.
133. The clinical significance of midplane pelvic contraction. Am. J. Obst., 1944, *48*, 52-57 (with P. C. Schumacher).
134. The growth and development of the pelvis of individual girls before, during, and after puberty. Yale J. Biol., 1944, *17*, 91-97 (with W. W. Greulich).

1945

135. The periodic gynecological examination. J. Connecticut M. Soc., 1945, *9*, 185-187.
136. Effects of severe rickets in early childhood on skeletal development in adolescence. Am. J. Dis. Child., 1945, *69*, 339-345 (with E. C. Dunham).
137. A case of incarcerated uterus successfully treated in 1808. J. Connecticut M. Soc., 1945, *9*, 771-772.
138. Some letters of Edmund Randolph Peaslee, B.A. Dartmouth 1836, M.D. Yale 1840. Yale J. Biol., 1945, *17*, 685-704.

1946

139. A plan to augment graduate medical training. J. Connecticut M. Soc., 1946, *10*, 26-27.
140. The obstetrical significance of pelvic roentgenometry. Interne, N. Y., 1946, *12*, 268-273.
141. A ten-year survey of Cesarean section at the New Haven Hospital. Am. J. Obst., 1946, *51*, 880-889 (with M. S. Godfried).
142. A discussion of pelvic variation and a report on the findings in 100 Negro women. Am. J. Obst., 1946, *52*, 248-254.
143. Outlet pelvimetry. A commentary and the presentation of a pelvimeter for measuring the "symphysis and sacral biparietal diameter." Surg. Gyn. Obst., 1946, *83*, 399-402.
144. The pelvic survey. Yale J. Biol., 1946, *19*, 171-179.

1947

145. The rooming-in plan for mothers and newborn infants. J. Connecticut M. Soc., 1947, *11*, 175-176 (with E. B. Jackson).

146. The role of nutrition in pelvic variation. Am. J. Obst., 1947, *54*, 62-73.
147. Roentgenography and roentgenometry of the pelvis. J. Mount Sinai Hosp. N. York, 1947, *14*, 653-658.

1948

148. A hospital rooming-in unit for four newborn infants and their mothers. Pediatrics, 1948, *1*, 28-43 (with Edith B. Jackson, R. W. Olmsted, A. Foord, and K. Hyder).
149. Puerperal subperitoneal hematoma: case report. J. Connecticut M. Soc., 1948, *12*, 419-421 (with Frederick W. Shull).
150. A clinical study of natural childbirth. Am. J. Obst., 1948, *56*, 875-883 (with F. W. Goodrich).
151. The rooming-in plan for mothers and infants. Am. J. Obst., 1948, *56*, 707-711 (with Edith B. Jackson, Lyman M. Stowe, and Frederick W. Goodrich, Jr.).

1949

152. Natural childbirth. J. Connecticut M. Soc., 1949, *13*, 47-48.
153. Natural childbirth in a teaching clinic. J. Obst. Gyn. Brit. Empire, 1949, *56*, 17-20.
154. A commentary on natural childbirth. Pediatrics, 1949, *3*, 613-616 (with F. W. Goodrich, Jr.).
155. Outlet pelvimetry: results in measuring the symphysis-biparietal and sacral-biparietal diameters in 145 primiparous women. Surg. Gyn. Obst., 1949, *89*, 67-68 (with Charles B. Cheney).
156. Training for childbirth. J. Am. M. Ass., 1949, *140*, 1256-1258 (with F. W. Goodrich, Jr.).

1950

157. Two instruments for cervical biopsy. Am. J. Obst., 1950, *59*, 222.
158. A study of midpelvic contraction. Am. J. Obst., 1950, *59*, 424-428 (with R. H. Wyatt).
159. *Training for childbirth.* A program of natural childbirth with rooming-in. New York, McGraw-Hill Book Co., 1950. xvi, 114 pp.
160. Training program for childbirth. Health News, Albany, 1950, *27*, 3-7.
161. Natural childbirth: a summary and a commentary on recent literature. Am. J. M. Sc., 1950, *219*, 679-683.
162. The infertility problem. A commentary and review of 3 years clinical experience. Yale J. Biol., 1950, *22*, 747-750.
163. A natural childbirth program. Am. J. Pub. Health, 1950, *40*, 787-791 (with R. H. Wyatt). (An abstract appeared in Obst. Gyn. Survey, 1951, *6*, 163-165).
164. *Understanding natural childbirth.* New York, McGraw-Hill Book Co., 1950.

1951

165. One thousand consecutive deliveries under a training for childbirth program. Am. J. Obst., 1951, *61*, 205-209 (with R. H. Wyatt).
166. Some Connecticut medical naturalists. J. Connecticut M. Soc., 1951, *15*, 339-341.
167. New Haven Hospital offers education for childbirth. Child, Wash., 1951, *15*, 163-165 (with E. Foord).

1952

168. Observations on training for childbirth. Given before the American Orthopsychiatric Association, Atlantic City, New Jersey, February 1952 in cooperation with the Department of Psychiatry. (with Lawrence Freedman). To be published.

THE LIBRARY OF JONATHAN KNIGHT
1789 — 1864*

JOHN F. FULTON

In 1940 Dr. Arthur Jackson of the faculty of the Yale School of Medicine brought to our attention the existence of a group of medical books in a barn in Litchfield, Connecticut. They belonged to a Litchfield resident named Mr. Joseph Cowan. Upon enquiry it was discovered that Mr. Cowan was the adopted son of Dr. J. Knight Bacon, whose mother was a daughter of Jonathan Knight, who at the age of twenty-four was appointed Professor of Anatomy when the Medical Institution of Yale College opened its doors in 1813. Dr. Harold S. Burr wrote an illuminating account of Jonathan Knight and his rôle in the founding and subsequent growth of the Medical Institution in 1929[1] but at that time was unable to ascertain the fate of his library. Dr. Burr was impressed by the fact that Knight seemed extraordinarily well informed on all phases of medical knowledge of his time and the assumption was that he had access to the latest publications. Knight himself stated that by 1838 the Library of the Medical Institution had a thousand volumes, but it seemed evident that in the early years of his professorship Knight must have had available an extensive personal library. Dr. Burr found that many early copies of the *Medical and Surgical Register* now in the Yale Medical Library bear Knight's signature, but nothing else from his library appeared to have found its way into the Yale collections.

There was considerable excitement, therefore, when it was discovered that the books in the barn in Litchfield bore Jonathan Knight's signature. They had apparently remained in the family since the time of Knight's death, but since no member of the present generation of descendents had followed medicine, Mr. Cowan had expressed a desire to dispose of the books. Unfortunately, several months before we heard of the collection a man representing himself as a bookseller had made off with a part of the library with the understanding that he would later pay Mr. Cowan on a commission basis. The "bookseller" has never been heard from again.[2]

The belief that Knight had closely followed the literature of medicine was fully vindicated by a study of the books that still remained in Mr. Cowan's

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* From the Department of the History of Medicine, Yale University School of Medicine.

[1] Jonathan Knight and the founding of the Yale School of Medicine. Yale J. Biol., 1929, *1*, 327-343.

[2] This is the second instance in recent years of a confidence man making off with valuable books in this region of Connecticut; hence, if any books bearing Jonathan Knight's name come to light, it would be worth while to trace their provenance.

barn. A list of 97 titles follows, comprising some 124 volumes; there are signs of positive identification in all but 26 volumes.

Study of the identifying autographs has proved of extraordinary interest. Many of the books had apparently come to Knight from his father, also named Jonathan, a practising physician of Norwalk, Connecticut. All of these bear numbers, presumably in the elder Knight's hand. The old gentle-

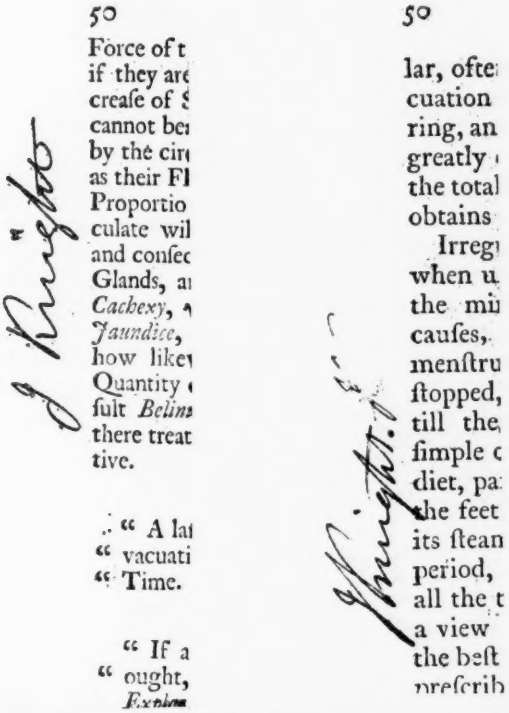

FIG. 1. Examples of the Jonathan Knight autograph occurring on page 50 of two of the volumes from the Knight library. On the left, the elder J. Knight; on the right, J. Knight, first Professor of Anatomy at the Medical Institution of Yale College.

man was evidently very cautious about his books, for not only did he sign them on the flyleaf (sometimes followed by "Norwalk Ct." and the date), but he also systematically signed them on page 50.

Jonathan Knight, Jr.,[3] upon inheriting his father's books, adopted the parental practice of signing them both on the flyleaf and again on page 50.

[3] Although Jonathan Knight did not use "Jr.," I have felt obliged to use the designations "Sr." and "Jr." to distinguish the Norwalk physician from his son.

Curiously, many of those he acquired from his father have his father's signature neatly cut out of the margin on page 50 and his own signature entered in another position along the margin. Just what his object in this could have been is impossible to determine, since his autograph is alarmingly similar to that of his father and few people would have been able to notice a difference. In fact, there are some instances where a handwriting expert would be required to determine whether the book had been signed by Jonathan Senior or Jonathan Junior. Several books carry the autograph of both father and son and thus give opportunity for comparison (Fig. 1).

One volume of especial interest is that carrying the signature of Dr. Asahel Fitch, father of Ann Fitch, wife of Jonathan Senior. This, curiously enough, bears the date 1789, the year in which the younger Jonathan was born. There are also two works carrying the autograph of "Jas. G. Knight," younger brother of Jonathan. One of these, John Innes's *A short description of the human muscles,* is interleaved and there are many manuscript comments. This is one of the few volumes containing annotations. Robert Thomas's *The modern practice of physic* is also interleaved with many annotations, and since the handwriting seems different from that of Jonathan Junior, the assumption is, in view of the James Knight autograph, that the annotations are by James and not Jonathan. James also studied medicine and started a practice in Stamford, Connecticut, but died young.

This collection of books brought together by one of the founders of the School gives an excellent idea of the state of medical practice and medical thought in the first half of the last century and it forms a particularly precious addition to our medical heritage at Yale. The volumes were acquired for the Library through the Harvey Cushing Fund soon after its establishment in 1940.

In examining the list, one should bear in mind the following dates: Jonathan Knight, Sr. was born in 1758 and died in March 1829. Jonathan Junior was born in 1789 and came to Yale in 1804 when fifteen years old. He was graduated in 1808 and took a Master's degree in 1810. From September 1810 he was a tutor at Yale for a year and then in the autumn of 1811 he went to Philadelphia to study medicine with Caspar Wistar and Benjamin Rush. There is evidence that Benjamin Silliman influenced Knight to abandon his tutorship and to prepare himself for a position in the projected school of medicine much as Silliman had been deflected from the law to become a professor of chemistry. From 1813 until 1838 Jonathan Knight occupied the chair of Anatomy and Physiology in the Medical Institution. From 1838 until his death in 1864 he was Professor of the Principles and Practice of Surgery. Many of the books listed below were acquired in 1812 to 1814 when he was in his early twenties. The inscriptions are included with the titles. The items preceded by a star are those which have a number on the flyleaf and appear to have been originally in Jonathan Senior's library.

ACCUM, Frederick. *A practical essay on the analysis of minerals.* . . . First American, from London edition. Philadelphia, 1809.
'J. Knight' on flyleaf and on p. 50.

BELL, Charles. *Institutes of surgery.* Edinburgh, 1838. 2 vols. [Vol. I only.]
'Knight' in pencil on flyleaf.

BELL, Charles. *The nervous system of the human body; embracing the papers delivered to the Royal Society on the subject of nerves.* Washington, 1833.

BELL, Charles. *A system of operative surgery.* London, 1807-09. 2 vols.
'J. Knight' on flyleaf and on p. 50 (small piece of page cut out of each vol.).

*BELL, John. *The anatomy of the human body.* From the fourth London edition, improved by the author. New York, 1809. 4 vols. in 2.
'$10. J. Knight, Norwalk, Ct. March 10th, 1810' on flyleaf of Vol. I. 'J. Knight' on p. 50 of Vols. I and III.

BICHAT, Xavier. *General anatomy, applied to physiology and medicine:* translated from the French, by George Hayward. Boston, 1822. 3 vols.
'J. Knight' on flyleaf and p. 50 of each vol.

BLAKE, Andrew. *A practical essay on . . . delirium tremens* . . . Washington, 1834. Bound with: C. Bell's *Nervous system,* 1833.

BLUMENBACH, J. F. *Elements of physiology;* translated from the original Latin, and interspersed with occasional notes. By Charles Caldwell. To which is subjoined, by the translator, an appendix, . . . relative to the subject of animal electricity. Philadelphia, 1795. 2 vols. in 1.
'$2. Jon Knight Norwalk Conn.' on flyleaf, and written below in pencil: 'This book belongs to Dr. Knight. It was borrowed by Francis D. Perry.' 'J. Knight' on p. 50.

*BLUMENBACH, J. F. *The elements of physiology.* Translated from the Latin of the fourth and last edition . . . by John Elliotson. Fourth edition. London, 1828.
'J. Knight' on flyleaf and p. 50.

BLUNDELL, James. *The principles and practice of obstetrics.* . . . To which are added, notes and illustrations. By Thomas Castle. Washington, 1834.
'J. Knight' on flyleaf and p. 50; 'J. Knight, New Haven' on 2nd leaf.

*BOERHAAVE, Hermann. *Aphorisms.* . . . Translated from the last Latin edition. . . . The third edition. London, 1755.
'J. Knight' on cover [flyleaf missing] and on p. 50 and p. 250.

BORELLI, G. A. *De motu animalium.* Leyden, 1710. 2 parts in 1.
'J. Newcome 1718' on inside of front cover; 'J. Knight' on flyleaf.

BRODIE, B. C. *Pathological and surgical observations on the diseases of the joints.* Third edition. Washington, 1834. Bound with: C. Bell's *Nervous system,* 1833.

BROOKES, Richard. *An introduction to physic and surgery.* London, 1754.
'J. Knight' on inside of cover [flyleaf missing] and on p. 50; on back flyleaf: 'Norton Book 1758,' crossed out and below it: 'Seth Warners Book 1767.'

CARNOCHAN, J. M. *A treatise on the etiology, pathology and treatment of congenital dislocations of the head of the femur.* New York, 1850.
'Dr. J. Knight with respects of the publishers' on flyleaf.

CARPENTER, W. B. *Principles of human physiology.* . . . Fifth American from the fourth and enlarged London edition. . . . Edited, with additions, by Francis Gurney Smith. Philadelphia, 1853.

CHAPMAN, Nathaniel. *Elements of therapeutics and materia medica.* . . . Second edition. Philadelphia, 1821-22. 2 vols.
'J. Knight 1822' on flyleaf of each vol., also pencilled signature with date 1834 in Vol. I; 'J. Knight' on p. 50 of both vols.

*CHISHOLM, Colin. *An essay on the malignant pestilential fever introduced into the West Indian Islands from Boullam, on the coast of Guinea, as it appeared in 1793 and 1794.* London, 1795.

'J. Knight Sep. 1811' on flyleaf, and 'J. Knight' on p. 50 [small piece of margin above signature torn out].

*CLEGHORN, George. *Observations on the epidemical diseases of Minorca. From the year 1744 to 1749. . . .* With notes by Benjamin Rush. Philadelphia, 1809.

'J. Knight Sep. 1811' on flyleaf and 'J. Knight' on p. 50.

COLLES, Abraham. *A treatise on surgical anatomy.* Part the first. Philadelphia, 1820.

'J. Knight New Haven 1821' on 2nd leaf, 'J. Knight' on p. 50. [Neither signature very characteristic.]

COOPER, Astley. *A treatise on dislocations, and on fractures of the joints.* First American from the third London edition. With notes and references by John D. Godman. Philadelphia, 1825.

'J. Knight 1825' on flyleaf, 'J. Knight' on p. 50.

COOPER, Samuel. *The first lines of the theory and practice of surgery . . .* with notes and additions by Willard Parker. New York, 1844. 2 vols. [Vol. I only.]

'J. Knight, M.D. with the respects of the publishers' on flyleaf.

COOPER, Thomas. *Tracts on medical jurisprudence.* Philadelphia, 1819.

'J. Knight, 1820' on flyleaf, 'J. Knight' on p. 50.

*COXE, J. R. *The American dispensatory. . . .* Philadelphia, 1806.

'J. Knight Sep. 1814' on flyleaf, 'J. Knight' on p. 50.

*CRICHTON, Alexander. *An inquiry into the nature and origin of mental derangement.* London, 1798. 2 vols.

'J. Knight Sep. 1811' on flyleaf of each volume, 'J. Knight' on p. 50 of both.

*CULLEN, William. *A treatise of the materia medica.* Edinburgh, 1789. 2 vols.

'J. Knight' on p. 50 of each vol.; 'Jonathan Knight' on 'Contents' page, and 'J. Knight' on p. 1 of Vol. II [these last two not characteristic.]

CULPEPER, Nicholas. *Pharmacopœia Londinensis.* London, 1683.

'J. Knight' on title-page and p. 50.

CUTTER, Calvin. *Anatomy and physiology: designed for academies and families.* Boston, 1846.

In pencil on 2nd leaf: 'Prof. Knight M.D. With respects of the Author. By Geo. S. Rawson.'

Cyclopædia of practical medicine. . . . Edited by John Forbes, Alexander Tweedie, John Conolly, and Robley Dunglison. Philadelphia, 1850. 4 vols. [Vol. I only.]

'J.W. Martin' in pencil on flyleaf.

DEWEES, W. P. *A treatise on the physical and medical treatment of children.* Tenth edition. Philadelphia, 1853.

*DORSEY, J. S. *Elements of surgery.* Philadelphia, 1813. 2 vols. [Vol. II only.]

'J. Knight' on front flyleaf and p. 50; 'J. Knight 1813' on last flyleaf.

DRAKE, James. *Anthropologia nova: or, a new system of anatomy. . . .* The third edition corrected. With a preface by W. Wagstaffe. London, 1750. 2 vols. [Vol. I only.]

DUNGLISON, Robley. *On the influence of atmosphere and locality . . . on human health; constituting elements of hygiène.* Philadelphia, 1835.

DUNGLISON, Robley. *Human physiology.* Philadelphia, 1832. [Vol. 2 only.]

DUPUYTREN, Guillaume. *Lectures on clinical surgery.* Washington, 1835.

FERRIER, John. *Medical histories and reflections.* Warrington and London, 1792-1795. 2 vols.

On flyleaf of Vol. I: 'J. Knight from Dr. Heermann'; on p. 15: 'Bought at auction of Wm. Leigh [name in previous owner's writing] by Lewis Heer-

mann M.D. to J. Knight, Esq. M.D. Professor of Surgery'; 'J. Knight' on p. 50. Vol. II, p. 1 has same inscription as p. 15 of Vol. I, except after "Hermann M.D." it says 'With his respects to Dr. J. Knight Professor of Surgery.' 'J. Knight' on p. 50.

FLEMING, John. *The philosophy of zoology.* Edinburgh, 1822. 2 vols. Front cover and flyleaf of Vol. I missing. 'J. Knight' on p. 50 of Vol. I and on flyleaf and p. 50 of Vol. II.

FREIND, John. *Emmenologia:* Translated into English by Thomas Dale. The second edition. London, 1752.
'J. Knight' and 'J. Knight's Book' [different writing?] on flyleaf; 'Jonathan Knight's Book' on 2nd leaf; 'Jona Arnolds' on title-page; 'J. Knight' on p. 50; 'Stephen Randal' on back flyleaf.

GARDNER, A. K. *The causes and curative treatment of sterility.* New York (1856).

GOOD, J. M. *The study of medicine.* Boston, 1823. 4 vols. [Vol. III missing.]
'J. Knight' on flyleaf and p. 50 of each volume.

GROSS, S. D. *Elements of pathological anatomy.* Boston, 1839. 2 vols.

GUY, W. A. *Principles of forensic medicine.* First American edition. With notes and additions by Charles A. Lee. New York, 1845.
'J. Knight Nov. 1846' on flyleaf.

HALLER, Albert v. *Elementa physiologiae corporis humani.* Lausanne and Berne, 1757-1766. 8 vols.

HAMILTON, Alexander. *A treatise of midwifery.* Second edition. Edinburgh, 1785.
'J. Knight' on flyleaf and p. 50. 'J. A. Walcott' stamped in gold on front cover.

*HEY, William. *Practical observations in surgery.* Philadelphia, 1805.
'J. Knight' on flyleaf and p. 50.

HUXHAM, John. *An essay on fevers. To which is now added, a dissertation on the malignant, ulcerous sore-throat.* Seventh edition. London, 1772.
'Jon^{n} Knight, Norwalk' on flyleaf; 'J. Knight' on p. 50.

INNES, John. *A short description of the human muscles. . . .* From the seventh London edition, revised and corrected by Alexander Monro, jun. New York, 1818.
Small book-plate of 'Jas. G. Knight' inside front cover; 'Jonathan [*sic*] Knight' on flyleaf (not characteristic signature); 'J. G. Knight' on p. 50. On 3rd blank page is written in ink:
"Who careless whirls from page to page
May make a fop but not a sage."
Below in pencil: "We know two or three examples. Dec. 30, 1828." Copy is interleaved and has numerous comments.

*JACKSON, Robert. *A treatise on the fevers of Jamaica.* Philadelphia, 1795.
'J. Knight Sep 1811' on flyleaf; 'J. Knight' on p. 50.

JACKSON, Samuel. *The principles of medicine, founded on the structure and functions of the animal organism.* Philadelphia, 1832.

Lancet. In two volumes. Vol. I. New York, 1843.

LAWRENCE, William. *A treatise on the diseases of the eye.* Washington, 1834.
'J. Knight' on flyleaf and p. 50.

LEEUWENHOEK, Antonius. *Epistolae physiologicae.* Delphis, 1719.
'J. Knight' on flyleaf.

LEEUWENHOEK, Antonius. *Epistolae ad Societatem Regiam Anglicam.* Leyden, 1719.
'J. Knight' on flyleaf.

LEEUWENHOEK, Antonius. *Opera omnia.* Leyden, 1722.
'J. Knight' on flyleaf.

*Le Gallois, C.-J.-J. *Experiments on the principle of life.* . . . Translated by N. C. and J. G. Nancrede. Philadelphia, 1813.
'J. Knight' on flyleaf and p. 50. "J. A. Walcott" in gold letters on front cover.

London Lancet. New York, 1846. Vols. 3 and 4 bound together.

London Lancet. New York, 1851. 2 vols in 1.

Louis, P.-C.-A. *Pathological researches on phthisis.* Translated from the French . . . by Charles Cowan. Washington, 1836.
'Dr. K' written in pencil on one of blank leaves in back.

Mackintosh, John. *Principles of pathology, and practice of physic.* First American from the third London edition. Washington, 1833. Vol. I only.
'Knight' in pencil on last page of index.

Massachusetts Medical Society. [Three incomplete parts of *Medical dissertations* and *Medical communications,* 1790-1821.]

Mayo, Herbert. *Observations on injuries and diseases of the rectum.* Washington, 1834. Bound with C. Bell's *Nervous system,* 1833.

Meckel, J. F. *Manual of general descriptive, and pathological anatomy,* translated from the German into French . . . by A. J. L. Jourdan and G. Breschet. Translated from the French with notes by A. Sidney Doane. New York and Philadelphia, 1831-32. 3 vols.
'J. Knight' on flyleaf and p. 50 of Vols. I and III.

**Medical and philosophical commentaries.* By a Society of Physicians in Edinburgh. Volume seventh. A new edition. Philadelphia, 1794.
'J. Knight Sep. 1811' on flyleaf, 'J. Knight' on p. 50.

Medical Society of the State of New York. Transactions. Albany, 1863.
'Jonathan Knight M.D. with the Highest respect of Tho. C. Brinsmade' on flyleaf.

Medico-chirurgical review, and Journal of Medical Science. [Quarterly.] London; New York, 1823. [Vol. II, for 1821-22.]

Miller, James. *The principles of surgery.* Philadelphia, 1845.
'J. Knight 1845' on flyleaf, 'J. Knight' on p. 50.

Monro, Alexander. *Elements of the anatomy of the human body in its sound state.* Edinburgh, 1825. 2 vols.
'J. Knight 1829' on flyleaf. 'J. Knight' on title-page and p. 50 of Vol. I; also on flyleaf and p. 50 of Vol. II.

O'Beirne, James. *New views of the process of defecation* . . . together with an analytical correction of Sir Charles Bell's views respecting the nerves of the face. Washington, 1834. Bound with: C. Bell's *Nervous system,* 1833.

Osborn, William. *Essays on the practice of midwifery, in natural and difficult labours.* London, 1795.
'J. Knight' on flyleaf and p. 50.

Philip, A. P. W. *On the influence of minute doses of mercury . . . in restoring the functions of health.* Washington, 1834. Bound with C. Bell's *Nervous system,* 1833.

Philip, A. P. W. *A treatise of febrile diseases* . . . First American from the second English edition. Hartford, 1809. 2 vols.
'J. Knight' on p. 50 of Vol. I; 'J. Knight Norwalk Ct. December 1809' on flyleaf of Vol. II, 'J. Knight' on p. 50.

Pirrie, William. *The principles and practice of surgery.* Edited with additions, by John Neill. Philadelphia, 1852.

*Quincy, John. *Lexicon physico-medicum: or, a new medicinal dictionary.* . . . The fifth edition. . . . London, 1736.
J. Knight' on flyleaf and p. 50.

*RICHERAND, A.-B. *The elements of physiology.* . . . Translated from the French by Robert Kerrison. Philadelphia, 1808.
'J. Knight New Haven Sep. 1811' on flyleaf, 'J. Knight' on p. 50.

*RUSH, Benjamin. *An account of the bilious remitting yellow fever, as it appeared in the City of Philadelphia, in the year 1793.* Second edition. Philadelphia, 1794.
'J. Knight N Haven' on flyleaf, 'J. Knight' on p. 50.

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ABSENCE OF HEPATIC INACTIVATION OF ESTROGEN IN THE MONKEY*

G. VAN WAGENEN AND W. U. GARDNER

Hepatic inactivation of estrogen is not a constant feature of the metabolism of all animals. In laboratory rodents (mouse, rat, and guinea pig) the liver effectively destroys the hormone when it is secreted by ovarian tissue transplanted to a region of portal drainage and, too, the crystalline form of the steroid is without effect when placed in a like position. On the other hand, Hooker, Drill, and Pfeiffer[4] found in the prepubertal monkey that implantation of pellets of estrone and estradiol in the spleen was followed by peripheral evidence of circulating estrogen.

The fate of the ovarian tissue transplanted to the spleen of rodents was of great interest. Granulosa-cell tumors, luteomas, and mixed cell tumors were reported (Biskind and Biskind, 1944; Li and Gardner, 1947).[2,5] The present writers, recognizing the possible deviation of the monkey from the pattern of estrogen conversion characteristic of the subprimate yet interested in the induction of tumor formation in a notably resistant animal, undertook transplantation of ovarian tissue to the spleen in both immature and mature monkeys. These transplants were referred to as "intrasplenic." It was the effort to achieve success for each graft in the larger and relatively more costly animal that led to the orientation of the ovarian segment in such a way as to preserve its normal relationship to the peritoneal cavity. Thus, the cut surface was approximated to the stroma of the spleen from which the capsule had been separated, and the surface of the ovary covered by germinal epithelium, was freely bathed by peritoneal fluid. Now, although the vascular supply of the surviving graft was surely a part of the portal system, there remained the question of possible hormonal secretion or of diffusion into the peritoneal fluid because the position of the graft was "suprasplenic" rather than "intrasplenic." It was recognized that the possibility of estrogen escaping passage through the liver should be checked, so truly intrasplenic grafts have now been made.

MATERIALS AND METHODS

Three rhesus monkeys (*Macaca mulatta*) have received intrasplenic transplants of ovary, two animals being adults and the third an immature monkey of about one and one-half years, weighing 3,200 grams. The adult monkeys were born in the colony and were 10 years and 3 months and 10 years, 9 months old. They weighed, respectively, 8,230 and 10,000 grams. One of them had been used for breeding and the other was a

* From the Department of Obstetrics and Gynecology and the Department of Anatomy, Yale University School of Medicine. Supported in part by a research grant from the National Cancer Institute, of the National Institutes of Health, Public Health Service.

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genetic female whose external genitalia had been altered by androgen treatment of her mother early in pregnancy. The pseudohermaphroditic change caused the vagina to end in the dorsal wall of the prostatic urethra, forming a male-like prostatic utricle that was patent. This animal matured at the usual age, the menstrual fluids passing through the utricle and externally by way of the urethra. The sex skin swelling and coloration appeared in response to ovarian activity at puberty, as in any individual of this species. She was satisfactory for this experiment except that the vagina was not available for the desquamation study which explains why her record is missing from the chart in Figure 3.

Although the hepatic inactivation of estrogens seems to depend on an adequate food and vitamin intake (Drill and Pfeiffer, 1946; Biskind, 1946),[8,1] no supplement to the balanced diet now used[9] was thought necessary. Since this colony is made up of pregnant animals, young growing animals, and monkeys subjected to operative procedures, a definite plan is followed to make sure that each animal receives and eats a supplement of vitamins A and D; the readily eaten oranges supply vitamin C and for B, Brewer's yeast is shaken on a slice of bread in the pan of milk each morning. Because the animals live entirely indoors a centrally located quartz, mercury-vapor ceiling lamp is used. Other possible inadequacies in the diet of animals preferring natural foods are covered by the addition of a salt mixture to cooked foods.

Transplantation. Nembutal anaesthesia was used, administered subcutaneously and in amount to immobilize the animal for several hours. For the preliminary castration of the animal a relatively high midline incision through the abdominal wall was made which later was extended in order to reach the spleen. The blood vessels to the ovaries were tied off and the ovaries removed without including the Fallopian tubes. The distal part of the fimbriated end of the tube was clipped to free it from the surface of the gonad. It has been customary to use one half of an ovary for the transplant, so after removal, the second gland was split lengthwise through the hilus toward the free border. The two parts were then approximated and the gland kept moist until the spleen was prepared.

The parietal surface of the spleen was brought into view by grasping the suspensory ligament. About midpoint on the splenic surface an incision of the capsule the width of the ovary was made and the knife blade was then turned at right angles deep in the stroma to cut a long pocket which lay parallel to the long axis of the spleen. Into this, one-half of the ovary was slipped and when the forceps was withdrawn, the edges of the incision fell into place, no stitch being necessary to insure retention of the transplant.

The two edges of the peritoneum of the abdominal wall were approximated by a running stitch followed by mattress sutures in the external fascia and subcutaneous closure of the skin. On waking, each animal was returned to its own cage so that on the next day the daily observations and routines could be followed as usual.

In transplantations formerly used, only the splenic capsule was cut and a probe was inserted under one edge with a sweeping motion to free the capsule from the stroma. The ovary was slipped under the capsule with its cut surface lying on the denuded splenic tissue, the ovary being pushed well beyond the entrance slit. The capsule covering the ovary was then torn to expose the germinal epithelium on the surface of the ovary. These superficial transplants were usually made on the visceral surface of the spleen to avoid possible adhesion to the abdominal wall.

One of the present series of monkeys with buried ovarian tissue was sacrificed five and one-half months after transplantation. The spleen was removed and sliced through the site of the graft and then photographed (Fig. 1). The calibration of the vaginal lavages, color of the sex skin, and menstrual histories of the other monkeys have been followed for eleven months.

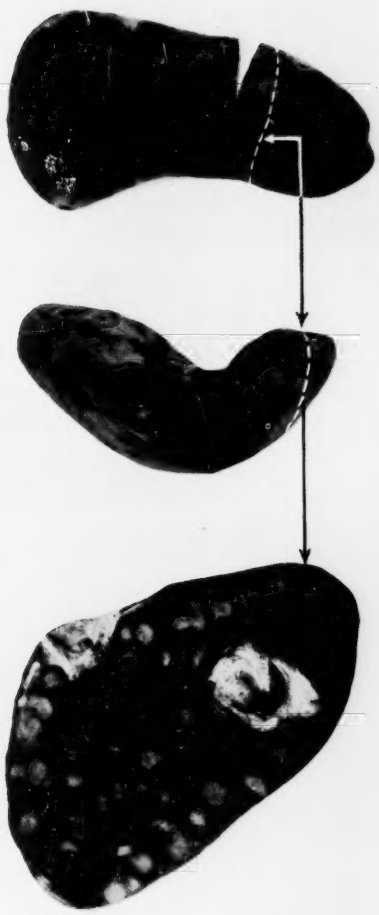

FIG. 1. Spleen from Mm 85, natural size, showing entire gland with initial cut through site of transplantation. Note its unbroken surface. The middle photograph shows the two parts of the spleen opened up to expose the embedded ovarian tissue. Lowest photograph is a surface view after a parallel cut, passing through a follicle, has been made deeper in the ovarian tissue (x 3).

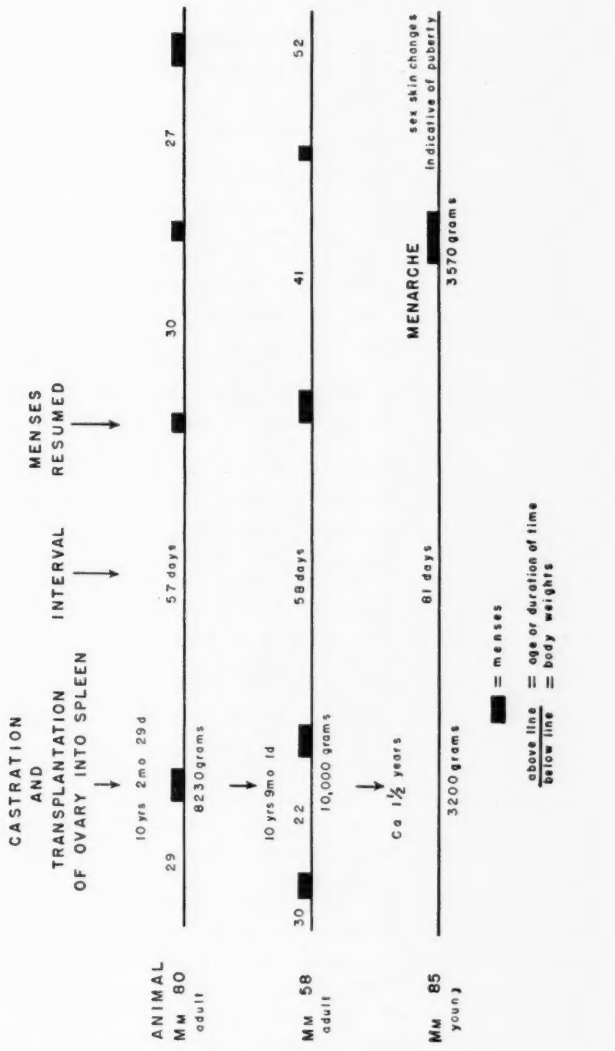

FIG. 2. Return and continuation of menstrual rhythms in ovariectomized rhesus monkeys bearing intrasplenic transplants of ovarian tissue.

Observations

After operation, the monkeys were essentially castrates until the transplanted ovarian tissue began to function. There was immediate evidence of this in the pallor of the sex skin of the two adults. Also, there was an effect on the sex cycle. Monkey Mm 80 was menstruating on the day of operation so the blood estrogen level was low, but Mm 58 was operated upon on the nineteenth day of the menstrual cycle and the hormone withdrawal resulted in a "castration" bleeding four days later, shortening the cycle to 22 days; the previous cycle was 30 days (Fig. 2). Immediately after operation upon Mm 80 the lack of estrogen support resulted in a profuse desquamation of the vaginal epithelium (Fig. 3). From this record it appears that the beginning hormonal secretion kept this desquamation from a complete fall to the baseline about twenty days after transplantation. In both adult animals the estrogen level was built up to normal during the following thirty days and the menses were resumed 57 days after operation in one animal and 58 days in the other. In both monkeys the color of the sex skin returned to normal.

In the immature animal, Mm 85, the nipples gave some evidence of development but the estrogen secretion of the gonads was too low to bring about appreciable vaginal desquamation. There was an initial rise in desquamation about two weeks after the operation (Fig. 3), indicating some ovarian activity but on too low a level to result in menstruation. The first menstruation (menarche) did not occur until after a second rise in hormone about forty days later, on the 82nd day after the operation. After menarche there was again a period of quiescence and then a rise in estrogen level. This pause after menstruation before the estrogenic or proliferative phase of the cycle is characteristic of the long cycles during puberty.

Gross appearance of the grafts. Mm 85, the animal which was immature at time of transplantation, was sacrificed five and one-half months later in order to examine the state of the graft. It was well embedded in the splenic tissue 2 to 3 mm. from the serosa (Fig. 1). A small whitish area beneath the splenic capsule indicated the location of the graft. Transection of the gland confirmed the intrastromal situation of it, passing through the periphery of a follicle; and a parallel cut toward the tail of the spleen revealed one follicle approximately 6 mm. in diameter. The follicle apparently contained clear liquor folliculi and had a translucent appearance. It was impossible to discern other ovarian structures from splenic stroma.

Microscopic structure of the graft. The sectioned graft measured approximately 9 mm. by 9 mm. at greatest diameters. It consisted predominately of a large follicle measuring 4.5 mm. by 5.5 mm. The walls of the follicle were irregular, in some areas containing 8-10 layers of granulosa cells and in other areas very few cells. No ovum was located. The theca interna was not well developed. One other follicle, approximately 1 mm. in diameter, was found. Luteinized granulosa cells and well-developed theca

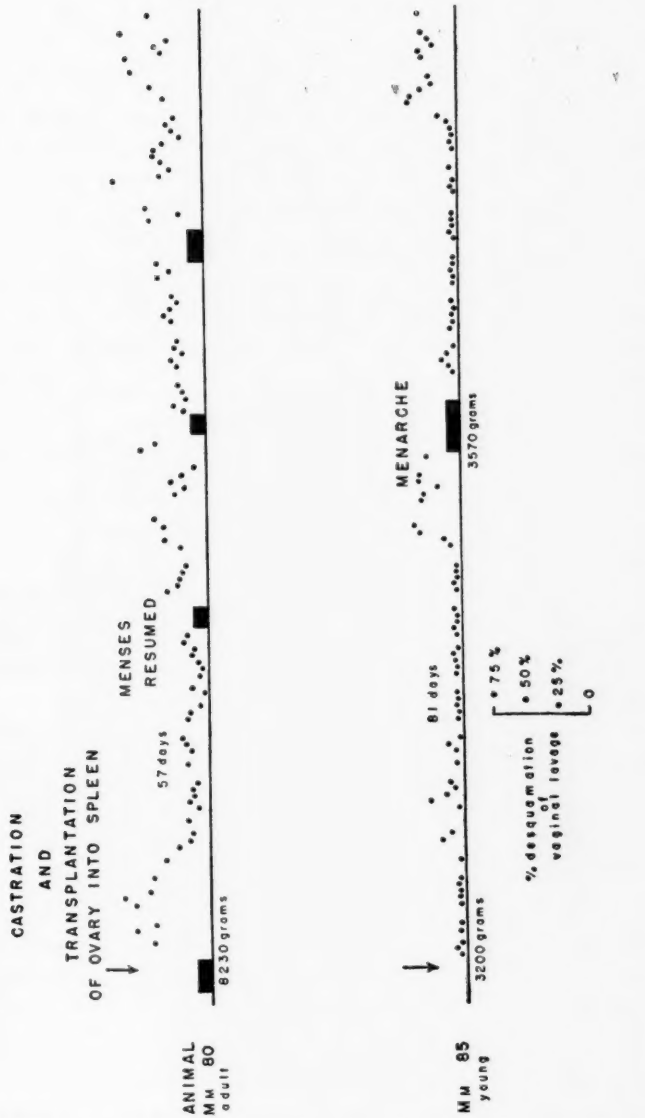

FIG. 3. Day-to-day fluctuation in vaginal desquamation, as determined by lavage, shown in relation to the onset and continuation of menses in adult and young ovariectomized rhesus monkeys bearing intrasplenic grafts.

interna lined the walls. An atretic ovum was located in a disintegrating cumulus oöphorus. Red blood cells were found in the lumen of one compressed follicle measuring 3 mm. by 2.5 mm. in greatest diameters.

Several small follicles, both normal and atretic, with one or two layers of granulosa cells were scattered about these larger follicles. In one area a cleft lined by germinal epithelium surrounded one part of the graft. A small area of rete ovarii and tubal epithelium was noted.

In no place did the graft contact the serosal surface.

Discussion

The continuation of cyclic vaginal bleeding of adult ovariectomized monkeys bearing intrasplenic ovarian grafts and the onset of menarche in an ovariectomized immature monkey bearing an intrasplenic ovarian graft reaffirm other evidence of the failure of the rhesus monkey's hepatic tissue to inactivate ovarian hormones (Hooker, Drill, and Pfeiffer, 1947; van Wagenen and Gardner, 1950).[4,7] The ovarian hormone, presumably an estrogen, produced by one-half of one ovary placed well within the splenic substance, stimulated the sex skin and vaginal epithelium as well as the endometrium. It seems impossible that any hormone could diffuse from the graft through the splenic capsule and serosa into the peritoneal fluids and hence escape the portal venous return.

The cyclic function of the intrasplenic grafts in ovariectomized monkeys, furthermore, was not unlike that of intact animals indicating the level of hormone production was adequate to maintain a sexual rhythm. This, as well as the vaginal desquamation and sex skin coloration, implies an essentially normal level of estrogen.

In the immature monkey from which the graft was obtained, corpora lutea were not found. Although the ovarian tissue was removed at a time of the year when ovulatory cycles would occur in adults, it is improbable that ovulation would occur in an animal of this age. The luteinization of the granulosa cells and hypertrophy of the theca interna indicate that the ovaries were exposed to some luteinizing hormone.

Summary and Conclusion

1. Autologous grafts of approximately one half of one ovary were made within the splenic tissue of one immature and two adult ovariectomized rhesus monkeys. After early indication of a hypo-ovarian condition as disclosed by the vaginal lavage and sex skin coloration, regular menstrual cycles were reëstablished in the two adult monkeys and the immature monkey became sexually mature at the appropriate weight.

2. One ovarian graft examined five and one-half months after implantation contained three follicles, two with luteinized granulosa cells and one with a resorbing ovum.

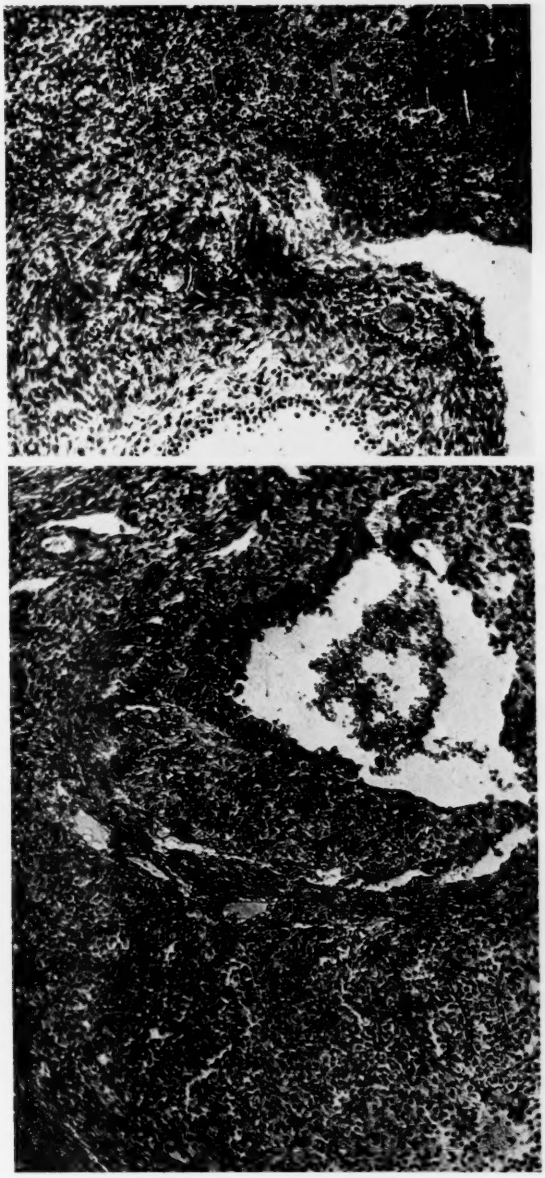

FIG. 4. Photomicrograph of an area of the intrasplenic graft in Mm 85 showing two small follicles with ova, one small anovular follicle, and below, a portion of the wall of a large cystic follicle. x approx. 100.

FIG. 5. Same ovary as above but showing a follicle with luteinized theca and atretic granulosa cells. Another section showed a degenerating ovum. Below is a section of a luteinized flattened follicle. No ovum was found.

3. Intrasplenic, not just subcapsular ovarian grafts, produce enough hormone to stimulate the sex skin, vaginal mucosa, and endometrium after the hormone has traversed the liver, giving further evidence that the monkey's hepatic tissue does not inactivate estrogens to any great degree.

ACKNOWLEDGMENTS

The assistance of Miss Dorothy Butzko and Mr. Joseph Negri at the operations and in the care of the animals is gratefully acknowledged.

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THE DEVELOPMENT OF ROOMING-IN AT YALE*

EDITH B. JACKSON

The term "rooming-in" refers to "a hospital arrangement for maternity patients wherein a mother and her newborn are cared for together in the same unit of space. However, its meaning reaches beyond physical facilities and signifies an attitude in maternal and infant care and a general plan of supportive parental education which are based on the recognition and understanding of the needs of each mother, infant, and family. It is a plan to maintain natural mother-infant relationships, to reinforce the potentialities of each mother and infant, and to encourage the family unit. From this broad point of view, then, rooming-in is not to be viewed merely as a specific plan for space arrangement or as a particular kind of equipment or organization, but rather as an integrated, interdepartmental program of professional assistance which is aimed to help parents achieve happy family unity and warm parent-child relationships. Such a program is developed through physician counseling, nursing procedure, personnel assignment, and hospital arrangement which, though they may vary in detail, are all based on this broader, deeper concept of the meaning of rooming-in."[15]

The foregoing statement defines the aims of the first *rooming-in programs* to be initiated in the United States. The definition was agreed upon in 1950 by representatives of medical centers in which rooming-in plans were in operation or in prospect. Active planning for rooming-in had begun in the early '40's at George Washington University,[17] in Detroit under the sponsorship of the Cornelian Corner,†[18] and at Yale University,[10] and interest was gradually extending to other institutions. The first scattered outcroppings of interest probably bore some relation to a series of interinstitutional and interdisciplinary meetings beginning in the late '30's concerning psychological aspects of child behavior and factors that favor or hinder healthy emotional development. Child care methodology thus came under review. Pediatric emphasis at that time was principally on regulations for physical care of children with little regard for psychological considerations. The selected group of child psychiatrists, psychologists, pediatricians, social scientists, and educators in these meetings shared the opinion that insistent strictness of routines in infant and child care played a rôle in the development of some of the emotional disturbances of childhood.

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* From the Departments of Pediatrics and Obstetrics and Gynecology, Yale University School of Medicine, the University Service of the Grace-New Haven Community Hospital, and the Yale University School of Nursing.

† An interdisciplinary group formed to advance the interests of parent-child relationships.

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Accordingly, attention was focused on ways of modifying child care practices in a less rigid direction.

Stimulus for such modification appears to have come also from mothers who expressed to their pediatricians a strong wish to have the baby in a closer and more natural relationship than was allowed by the usual hospital maternity arrangements. In Washington, D. C., Mrs. Frances Simsarian appealed to Dr. Preston McLendon to facilitate hospital arrangements which would allow her to breast feed her baby in response to his hunger rather than the clock. The arrangements called for having mother and baby in the same room. They were sanctioned on the basis of permitting scientific observations of an infant's reaction on a "self-demand" schedule. The report of this "experiment" by Mrs. Simsarian and Dr. McLendon in itself[2] and in its subsequent influence[3] documents the scientific introduction to hospital maternity rooming-in. At approximately the same time that Mrs. Simsarian was making her request, other mothers elsewhere were doing likewise. In New York, Dr. Margaret Mead asked Dr. Benjamin Spock's help in arranging for her newborn baby to be in the same room with her, and in New Haven Mrs. Joan Erickson asked for a similar arrangement through her obstetrician, Dr. Margaret Tyler, her family counselor, Dr. Marian C. Putnam,* and her husband, Erik Erickson, a Yale faculty member in the Department of Psychiatry. Although these two maternal requests were not the subject of official report, they were nonetheless influential. The difficulties these parents met in gaining permission for the desired hospital arrangements forcibly directed the attention of their professional associates to the disruptions of natural family relationships caused by rules and routines of hospital maternity care. One of these associates at Yale was Dr. Grover F. Powers, Chairman of the Department of Pediatrics.

Dr. Powers, ever alert to ways of improving service to patients and their families, picked up the challenge for change in the pediatric approach to child guidance and for integration of the pediatric and obstetrical responsibilities in hospital maternity service. In a paper which appeared in this Journal fourteen years ago he stated:

> . . . It may be appropriate to mention a matter which gives me concern—the interrelated responsibilities of obstetrician and pediatrician in the handling of mother and baby during the neonatal period. Many obstetricians turn over to the pediatrician the care of the newborn baby. I approve heartily of this procedure, but I am sure it is not entirely satisfactory at this time due to the fact that many pediatricians do not utilize the obstetricians' special knowledge of and relationship to the mother at this critical time. He very naturally has the special confidence of the mother of the baby he has just brought into the world. . . . The pediatrician too often comes into the situation almost a stranger. And yet, just at this time the adjust-

* Psychiatric consultant to the Department of Pediatrics. When Dr. Putnam left New Haven in 1939, the writer succeeded her in this interdepartmental relationship.

ment between mother and new baby—one of the most delicate and critical either will ever have to face—has to be made. This adjustment cries out for complete co-operation and understanding between the two physicians and for a unified program as to management and maternal nursing. [And again] . . . Certain developments in pediatrics itself have had malignant influence upon the emotional life of children. . . . Rules and regulations have their proper place as rough guides against too flagrant a departure from the ordinary routines of life and conduct. But insistence upon fixed procedures without wide latitude for individual variation has brought upon many children emotional disorders which have manifested themselves in a variety of "body protests" and of other displays of neurotic behavior. Our strict therapy has often fostered some of the very disorders we were trying to prevent.[4]

To implement the indicated changes, Dr. Powers sought ways of providing his Resident staff with opportunities and responsibilities in the supervision of well babies. With the co-operation of the city health officer, Dr. Joseph I. Linde, he was able in 1939 to introduce a home visiting plan for the nursery interne for the follow-up of clinic-status neonates. This plan had the dual aim of providing needed service for clinic mothers and realistic experience for pediatricians-in-training beyond the impersonal routines of the hospital.[9] Similarly, his associate, Dr. Herbert Miller, who was then physician-in-charge of the pediatric out-patient clinic and the newborn nursery, encouraged a more general use of the six-week check-up examination and the child health conference* to implement the individual approach in well baby care in the training program of staff and students. And periodically, during the early 1940's, Dr. Powers let fall the suggestion that rooming a baby together with his mother be tried. The freedom to make this suggestion had been given him by Dr. Arthur T. Morse, Chairman of the Department of Obstetrics and Gynecology.

The fact that it was only in 1944 that the suggestion was first carried out may be largely explained by the shortage of hospital staff and the accelerated teaching programs during the war years. A theoretical description for "a rooming-in arrangement for the baby" had been made to the reading public in 1943 with the publication of *Infant and Child in the Culture of Today* by Dr. Arnold Gesell and Dr. Frances Ilg.[5] The term, "rooming-in," thus came into general use, and the interest of many patients in New Haven and elsewhere was stimulated.

The early attempts with rooming-in during 1944 and the summer of 1945 revealed definite interest in rooming-in among some parents and members of the hospital staff, and they pointed up the need for close interdepartmental co-ordination for future undertakings. To this end, Dr. Morse and Dr. Powers arranged for occasional joint meetings of the obstetric and newborn nursery staffs. In addition, the combination of events during the following years gradually effected a co-ordination of interest.

* Held by the Visiting Nurse Association of New Haven and the New Haven City Department of Health in the pediatric out-patient clinic.

Dr. Alan Foord, Dr. Edith B. Jackson, and Dr. Herbert Thoms—three members of the Rooming-in Policy Committee. The snapshot was taken in October 1946 by Miss Elly James, a Swedish newspaper reporter on a visit to the United States, who had come to New Haven to "look at the rooming-in method" and report on it for her paper.

In July 1945, Dr. Alan Foord became instructor in pediatrics and public health and physician-in-charge of the pediatric out-patient clinic, and the writer became physician-in-charge of the maternity nursery and the neonatal follow-up clinic.* They shared responsibility for furthering the program of teaching around the well baby services in the hospital. The general lack of enthusiasm of the hospital staff for this aspect of the work seemed due in part to a lack of continuity in their relationship to families. The home visiting plan for the nursery interne had been the first step in providing him with some continuity in experience, but the two-month period of rotation was too short. It had long been noted that the pediatrician was in the key position to make detailed observations of mother-child relationships and had special need to understand them. Accordingly, Dr. Powers suggested the initiation of a study in this field under the writer's supervision, with a pediatrician-in-training as a research assistant and the framework of the study so devised that it could both serve the interests of mothers and infants and provide training for the annually appointed pediatric assistant. When the initial formulations for such a study were made in the fall of 1945, it was hoped that it might become associated with and strengthen the development of rooming-in. However, the future of rooming-in at Yale was uncertain just then.

Early in 1946 plans for a new building of the Grace-New Haven Community Hospital were getting underway. Dr. Albert W. Snoke had been appointed director of the hospital. The question was raised with him about the advisability of incorporating rooming-in arrangements in the architectural plans. His immediate reaction, with no prior knowledge of the background of the question, was adverse.† However, he allowed himself to be convinced that the question should be given further consideration. Dr. Powers, feeling perhaps that a good idea was seriously threatened, wrote in a letter to Mr. Robert S. Judd, Chairman of the Building Fund Committee, on March 1, 1946, as follows:

> . . . In any building operation, time always reveals that mistakes have been made. These are often unavoidable, but I am absolutely convinced that if we do not set up some units in the new hospital to meet the need of those mothers who wish to have their babies with them from birth on, we will be making a mistake in full knowledge at this time of the fact that we are making a mistake. The maternity unit should have great flexibility. . . . Mothers in this community are definitely interested, and this interest is increasing. . . .

On March 4, 1946, Miss Kate Hyder came to Yale as the newly appointed nursing supervisor of the obstetric and gynecologic service and assistant professor of obstetric and gynecologic nursing. Four days later

* Dr. Miller resigned to become chairman of the Department of Pediatrics at Kansas University School of Medicine.

† Dr. Snoke has often quoted himself as saying, "That's as crazy an idea as I've ever heard!"

the pediatric nursery staff were invited to join the obstetric staff at a tea meeting for an informal discussion of breast feeding, "self-demand," and rooming-in. Dr. Herbert Thoms, then associate professor of obstetrics and gynecology, was chairman. He gave enthusiastic support to the rooming-in idea. "Let's go!" he said. Miss Hyder expressed definite interest, but she asked for time to study the situation, knowing that the ultimate success of a rooming-in plan must depend on the adequacy and attitude of the nursing service. The decision for immediate plans on the maternity floor and for future plans in the new hospital building awaited Miss Hyder's conclusions.

In the meantime (April, 1946) Dr. Powers made a request to Mead Johnson & Company for "a grant-in-aid in support of an investigation in mental hygiene as respects parents and neonates." He submitted a plan for a three-year study in the Department of Pediatrics and defined its aim as follows:

> The objective of this project is to obtain necessary knowledge for the guidance of parents in order to prevent or at least reduce the great number and variety of behavior problems and psychosomatic disorders in young children, some of which are nutritional in nature and often apparently have their inception in the early days or weeks of life. Physicians, especially obstetricians and pediatricians, are continually confronted with the necessity of giving concrete advice and direction to women, for which there is as yet little scientifically based knowledge for their guidance in the subtle but highly significant mental health aspects of infant care. We believe that a study of the interrelation between maternal attitudes and the development of behavior patterns in children will lead to a further understanding of the origin of certain behavior disorders and therefore to the possibility of their prevention.

The requested budget was in support of a full-time special supervising nursing assistant or fellow, a full-time pediatric research assistant or fellow, and special nursing or other service necessary for acquiring and for recording data.

Notification of the acceptance of this application was received in May. By this time Miss Hyder had reached the conclusion that rooming-in could not be successfully carried out in single rooms, but that it could be worked out in a unit with four mothers and babies with the existing quota of nursing personnel plus one additional graduate nurse to be in charge of the unit. Dr. Powers suggested the prospective Mead Johnson Nursing Fellow for this position. In this way the study plan became linked with the rooming-in program. The appointment of a full-time Mead Johnson Pediatric Fellow* for the ensuing academic year was made almost immediately to assist in the study and in the eventual rooming-in plan.

In June 1946, the group of individuals most closely involved in the development of rooming-in met together in the writer's office to initiate

* Dr. Edward Daniels, called to military service in August 1946, was replaced by the appointment of Dr. Richard W. Olmsted in September 1946.

plans in accord with Miss Hyder's recommendations and Dr. Powers' suggestion. Those present were Dean Elizabeth Bixler of the Yale School of Nursing, Dr. Alan Foord,* Miss Kate Hyder, Dr. Grover Powers, Dr. Herbert Thoms, and the writer. (This group subsequently formed itself into the Rooming-in Policy Committee.) The first action was a request to Dr. Morse for permission to use the maternity ward solarium for rooming-in. This was in the days before early ambulation and the solarium was actually little used except as overflow space during periods of high census. Permission was immediately granted. The request to remodel the solarium was then made to Dr. Snoke who had in the meantime, under the impact of the interdepartmental solidarity, become definitely interested in the possibilities of rooming-in and was willing to have it tried out as a hospital "experiment." In July, the necessary changes in the solarium were authorized. Dr. Martha L. Clifford, Director of the Bureau of Maternal and Child Hygiene of the Connecticut State Department of Health, helped with the plans for remodeling.

While the conversion of the solarium was in progress (September and October), the first stages of the parent-child relationship study began to crystallize around plans for the rooming-in unit and the work of the pediatric fellow, Dr. Richard W. Olmsted. The latter was assigned responsibility for the care of the infants with their mothers in the unit and for observations for the study. Methodology for the recording of his initial observations were worked out by Dr. Ethelyn H. Klatskin,† Dr. Seymour Sarason,‡ Dr. Powers, the writer, and Dr. Olmsted himself. It was planned that he should select mothers in prenatal clinic for rooming-in, that he should prepare the selected mothers for the "ad-lib" procedures of rooming-in, and at the same time record his observations for the study regarding the mothers' attitudes and responses. He would then continue his dual rôle of pediatric adviser and observer in the rooming-in unit, where he would be assisted by the nursing fellow, and postnatally in home visits, neonatal clinic, and child health conference. Plans for the rooming-in unit and plans for the study were thus merged, and all aspects of the work were thought of as belonging to the development of a rooming-in program which offered continuity in counselling service for parents and in experience for the pediatric fellow. Everything connected with this program was labeled "rooming-in." Accordingly, when the rooming-in unit was opened at the end of October 1946, "rooming-in mothers" were waiting for admission and were followed by the "rooming-in fellow" according to the "rooming-in pro-

* Dr. Foord had been appointed an assistant director of the Grace-New Haven Community Hospital in April 1946.

† Dr. Klatskin has been research assistant in pediatrics (psychology) since 1946, and is assistant professor of psychology.

‡ Dr. Sarason was at the time psychologist at the Southbury Training School and assistant attending psychologist on the University Service of the Grace-New Haven Community Hospital. At present, he is associate professor of psychology at Yale.

gram" of study. Because of the joint framework of plans for the rooming-in unit and for the parent-child relationship study, the latter acquired the title "Rooming-In Project."*

This identity in name gave rise to confusion as to the nature of the rooming-in "experiment" and encouraged the erroneous assumption that the research was directed toward proving the advantages of rooming-in over traditional methods of hospital maternity care. The early history of the cross-fertilization of clinical and study procedures relating to the rooming-in unit has been reviewed in detail both to clarify this misunderstanding and to indicate the variety of influences which contributed to the vigor of rooming-in at Yale. The training opportunities of the program appealed to Dr. Powers so much that he assigned as many pediatric fellows of assistant resident rank as he thought it could absorb while data for the study were being collected. He appointed two fellows for the second year and three for each of the following three years.† One of the fellows, Dr. Morris A. Wessel, remained associated with the rooming-in program for three years.‡ Since the Rooming-In Project study,§ per se, is not the main theme of this paper, I shall continue with an outline of the development of rooming-in on the University Service of the Grace-New Haven Community Hospital.

There was unanimity about the over-all aims and procedures for the rooming-in unit.[14] These may be summarized as follows: The room should be cheerful, comfortable, quiet, and homelike. There should be an adjacent night nursery for the infants to protect the mother's sleep against interruptions except for breast feeding. There should be a nurse available around the clock to meet the needs of both mothers and infants on a flexible schedule. Nurses and doctors assigned to rooming-in should have prior orientation as to the rationale of "ad-lib" methodology, and to management of breast feeding. Mothers should have prenatal orientation to hospital facilities and opportunity for prenatal discussion with the physician who would supervise the infant's care. Only mothers who wanted rooming-in should be assigned to the unit. Mothers might be admitted to the unit directly from the delivery room if there were an available bed; otherwise, in turn as rooming-in beds become available. Each mother should have the

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* The initial grant-in-aid from Mead Johnson & Company terminated in 1949. The Project has received short-term grants-in-aid from The George Davis Bivin Foundation, Inc., The Field Foundation, Inc., and a continuing grant from the National Institute of Mental Health of the National Institutes of Health, United States Public Health Service, for the five-year period 1948-1953.

† The Fellows succeeding Dr. Olmsted were as follows: James A. Kleeman, Ruth I. Svibergson (1947-48); George R. Barnes, Jr., Anton N. Lethin, Jr., Morris A. Wessel (1948-49); John C. Cobb, Richard H. Granger, Sylvia N. Preston (1949-50); George S. Harris, Gordon Jensen, William L. Nyhan (1950-51).

‡ Dr. Wessel was Fellow of the Dazian Foundation for Medical Research 1949-50 and Senior Pediatric Fellow 1950-51.

§ Preliminary reports of this study have been published.[12, 13] A more inclusive report of methodology and a report of a statistical analysis of the relationship of maternal attitudes in child care practices and the child's behavior in 50 mother-child couples is in process of preparation by Dr. E. H. Klatskin and the writer.

opportunity to learn about the care of her infant, first by observation of the nurse, and then by practice under the nurse's supervision. A mother should not be expected to take care of her infant until she felt ready to do so. The father should be allowed to visit his wife and baby together and share the parental satisfaction of holding his infant and learning general aspects of infant care. Precautions similar to those of attending physicians should be carried out by visiting fathers before holding the baby. Daily visiting hours should be arranged. (Fortunately, it developed that they could be arranged twice daily.) Visitors should be limited to the father and two close relatives or friends for the duration of the rooming-in stay, and only one of these visitors should be allowed at a time.

The rooming-in unit was opened and maintained with few exceptions on the foregoing principles and there were usually more than enough applicants to fill it. It proved an immediate success, judging by the appreciative response of participating parents, nurses,[2, 7] and doctors.[19] The mothers' appreciation was noted in their attitude and conversation in the unit, their enthusiastic remarks to the unit's professional visitors, their favorable reports on comment cards returned to the hospital director's office, in their "impressions" written for the rooming-in staff, in their zeal for writing up their experience,[1, 10] and for participation in panel discussions, and finally in their eagerness to return with subsequent babies.

With the successful realization of plans there developed a group feeling of parenthood for the unit and a sense of pride in its performance. Dr. Thoms, whose enthusiastic sponsorship for rooming-in on his* maternity service has been noted above, felt that rooming-in really belonged to obstetrics, and should be promoted by obstetricians principally through obstetric channels.[27] Miss Hyder, with her tremendous zeal for "revitalizing" the teaching and practice in obstetrical nursing, regarded the rooming-in unit as a direct means to this end and staffed the unit as far as possible with student nurses.[8] Dr. Powers and the writer, through their close association during the preliminary planning years, saw the unit as a means of introducing pediatricians-in-training to an important aspect of their future work, but primarily as a measure for wholesome family integration and avoidance of anxieties and misunderstandings that arise when family members are kept apart.[11, 23] Dr. Snoke and Dr. Foord regarded it as an indicator for future architectural and administrative plans.[24, 25] The early favorable responses indicated probable continued success for the "experiment" and encouragement for eventual expansion.

Very soon after this heartening start, developments in the Department of Obstetrics began which added greatly to the effectiveness of the rooming-in program. Three mothers who had expressed a wish to have rooming-in

* In 1947, Dr. Thoms was appointed professor of obstetrics and gynecology, and obstetrician- and gynecologist-in-chief of the Grace-New Haven Community Hospital University Service.

independently approached their obstetricians with a request to have natural childbirth also. They had read either Grantly Dick Read's book[3] or the popular account[4] of his theory and the application of it in this country by Dr. Blackwell Sawyer. They won sympathetic interest from their respective physicians, Dr. Thoms, Dr. Frederick W. Goodrich, Jr.,* and Dr. Margaret Tyler.† All three babies were successfully delivered by the "Read" method in January 1947, at the very time when Dr. Dick Read was in this country at the invitation of the Maternity Center Association of New York. When he came to New Haven for a lecture to Dr. Thoms' staff and students, he was taken to see the rooming-in unit. It was a pleasant coincidence that one of the three mothers who had followed suggestions from his book, Mrs. Olmsted, wife of the rooming-in fellow, was there to greet him and report her success with his method.

Thus, again, the desire of mothers for a more natural approach to hospital maternity care, coupled with a sympathetic professional response, started modifications in obstetric procedure. These changes, together with the rooming-in plan, grew into an integrated program of maternity education as defined in the introductory quotation. Dr. Thoms, with the able assistance of Dr. Goodrich[6] and Miss Hyder, initiated the program. This became well established during 1948. Dr. Thoms assembled the details of the program in book form in a remarkably clear and graphic way under the title, *Training for Childbirth: A Program of Natural Childbirth with Rooming-in.*[5] It was immediately obvious that the two programs were "part and parcel' of each other, that they shared a basic philosophy of parental education, with emphasis on parents' participation in procedures and in knowledge about processes which affect them and their infants. The integration of the two programs brought the answer to Dr. Powers' plea for co-operation and understanding between obstetricians and pediatricians for "a unified program as to management and maternal nursing." Dr. Wessel worked in close liaison between the resident obstetric and pediatric staffs and was active in the teaching program for nurses and for parents in both the training for childbirth and the rooming-in programs.

The growing popular and professional support of the integrated programs put pressure on the administration to expand rooming-in facilities. The addition of another unit in October 1948‡ made it possible to accommodate the majority of mothers registered in prenatal clinic who wanted rooming-

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* Assistant resident in obstetrics and gynecology.

† In private practice, and associate clinical professor of obstetrics and gynecology.

‡ This was first set up as a five-bed unit in the interests of "A Time Study of Nursing Activity in Rooming-in." This study was under the sponsorship of the Department of Health of the State of Connecticut, assisted by the Children's Bureau of the Federal Security Agency. It was carried out under the direction of Barbara Lucas Holman, R.N., graduate student in the Department of Public Health. The report has not been published, but copies are available for reference. When the Time Study was completed, the five-bed unit was reduced to four beds.

in. There was also pressure from some families who had private obstetricians, especially those affiliated with the medical school and hospital. In the absence of rooming-in accommodations in the private pavilion, a monthly quota of six private patients for the rooming-in unit was approved by Dr. Thoms and Dr. Snoke. The private patients who wanted rooming-in were so eager to assure themselves a reservation that they applied very early—a few as soon as they thought they were pregnant but before the diagnosis had been confirmed by their obstetricians! The quota for any given month was usually filled six months ahead. For the numerous private patients who had to be disappointed, the promise of a new hospital building with facilities to accommodate all mothers who wanted rooming-in was definitely made.

The units were maintained in continuous operation for six and four years respectively, closing only for occasional days during low census periods or for redecoration. The combined response of parents and staff continued to be consistently favorable. Rooming-in facilities in several hospitals elsewhere were modeled after the New Haven Hospital units, and similar observations were reported.[30] That the units continued to be spoken of as "experimental" was due to the original association with the Research Project.

As the Research Project moved into its final phase, the rooming-in units gradually became independent of it. The nursing fellowships ceased in June 1950,* and the pediatric fellowships in June 1951. For the following academic year (1951-52), a grant to the Department of Pediatrics† permitted the appointment of two clinical fellows to continue the service aspects of the rooming-in program. These could not be carried on adequately by the existing resident staff without additional help. For the year 1952-53, Dr. Milton J. E. Senn‡ recommended the appointment of two additional pediatric assistant residents to conduct the neonatal and well baby services in the hospital.

With this change, the rooming-in set-up came into its own as a hospital service. It continued to provide the same general satisfactions as it had in its more protected days. It had hardly become used to its independence when the transfer on February 23, 1953 of all maternity patients from Dr. Thoms' service and the private pavilion to the new Memorial Unit of the Grace-New Haven Community Hospital introduced rooming-in at Yale to its major test of maturity.

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* The four nursing fellows were Mrs. Carolyn Krementz, Miss Nilda Shea, Miss Madeline McKenna, and Miss Gail Graham.

† From the Children's Bureau of the Federal Security Agency.

‡ Dr. Senn, Sterling Professor of Pediatrics and Psychiatry, on the retirement of Dr. Powers succeeded him as pediatrician-in-chief of the Grace-New Haven Community Hospital University Service.

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THE SOCIAL HISTORY OF THE CAUL*

THOMAS R. FORBES**

L'imagination engendre la superstition, la raison engendre la science.[15]

The history of obstetrics, perhaps more than that of many sciences, includes a large measure of superstition. In earlier centuries the normal phenomena of pregnancy and labor were understood, if at all, incompletely; related abnormalities were even more puzzling. Before adequate scientific explanations were available, speculation had to suffice, and superstition was often a result.

Infrequently, in past ages as now, a baby is born with a thin, translucent tissue, a fragment of the amniotic membrane, covering its head. The remnant is known as a *caul*.[5] The modern obstetrician quickly removes the membrane (it may be interfering with the infant's efforts to begin respiration) and discards it. His professional predecessors, the physicians and midwives of earlier centuries, would have been more interested, for strong magic and strange beliefs were once related to the caul.

In countries all over the world it was expected that the membrane would bring fame and fortune to its owner.[9, 34, 43, 50, 97, 80, 107] Aelius Lampridius, a classical historian, related that the emperor Antonius Diadumenianus or Diadematus (born 19 September 208 A.D.) was so called because at birth his head was encircled with a fillet (*diadema*), twisted like a bowstring and so strong it could not be broken.[33, 35, 63, 69, 86] One supposes that the caul in this case had been rolled into a band. Although the possession of the fillet was expected to bring him good luck, Diadumenianus was assassinated while a youth.[101] Caul superstitions were recorded again after the Dark Ages. Cornelius Gemma, a sixteenth century physician, scorned belief in the powers of the caul.[39] He described it quaintly as being ". . . nothing other than the remnant of another membrane, much softer than the amnion, but nevertheless more solid, bound with a purple border or fringe, and wrapped around the whole head down to the umbilicus, not without great danger to the baby unless the membrane was removed as quickly as possible; thus I myself have observed it in my first-born son who came helmeted [*galeatus*] into the world." Van der Spiegel, a Belgian anatomist, explained

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* From the Departments of Anatomy and the History of Medicine, Yale University School of Medicine. Read 17 October 1952 before the Beaumont Medical Club. The author is indebted to Professor G. Lincoln Hendrickson of Yale University for translating two passages and to the reference staffs of the Medical Library and Sterling Library for assistance in locating much material.

** Secretary-Treasurer, The Beaumont Medical Club, 1950-. The Beaumont Club has had only three Secretary-Treasurers. Dr. Samuel Harvey served from 1921-33 and Dr. Thoms, one of the founders, from 1933-50.

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how the amniotic membrane may in childbirth "be draped partly or completely around the head, so that in the male it may be called a helmet [*galea*] by the German obstetricians, or in the female, as the Italians put it, a fillet [*vitta*] or shirt" [*indusium, camisia*].[89] He suggested that it is ridiculous to attach supernatural significance to a phenomenon which can be readily explained.

Charles Drelincourt (1633-1697), another physician, wrote *De Foetum Pileolo sive Galea* [On the Little Cap or Helmet of Fetuses].[38] Unfortu-

FIG. 1. A sixteenth century concept of the caul, from Gemma's book.[89] See text.

nately, his rather extended discussion consisted largely of harsh sarcasm directed at the ideas of some of his colleagues regarding the caul. Drelincourt made it clear that he too rejected any idea of the membrane's power to bring good fortune.

In the following century, Thiers, in his interesting *Traité des superstitions,*[95] quoted the French proverb that the happy man was born with a veil (*est né cöeffé*) and expressed the opinion that any advantage coming to one who wears the caul as a charm could only be with the help of the Devil. Haller, the Swiss physiologist, made passing reference in his *Physiologie*[48] to the caul as a supposed sign of good luck. Thomas Bartholin scoffed at the superstitions because, he said, he had known unlucky children who were born with cauls and lucky children who were born without them.[12]

Spindler,[90] another physician of the period, said that he had never heard of a caul worn as a charm bringing good luck, but then told of a male patient who ascribed his impotence to the fact that his wife wore such a charm, i.e., the caul could have an evil influence. In Hungary it was darkly prophesied that he "who is born in the cloak [*Hulle*] dies in the rope."[50]

The beneficent effect of the caul was sometimes regarded as extending to the offspring of the original owner, but, according to a superstition of the Middle Ages and later, this effect would be lost if the caul were given away or sold outside the family.[4, 50] On 20 May 1658 the will of "Sir John Offley, Knight of Madely Manor, Staffordshire" was probated. The document includes the following bequest: "Item, I will and devise one Jewell done all in Gold enammelled, wherein there is a Caul that covered my face and shoulders when I first came into the world, the use thereof to my loving Daughter the Lady Elizabeth Jenny, as long as she shall live. . . ."[23] It was directed that the caul be passed on to the males of succeeding generations and that "the same Jewell be not concealed nor sold by any of them." One notes that the bequest refers not only to the possession but also to the "use" of this curious heirloom.

Lemnius discussed the caul in his *De Miraculis . . . Naturae,* first published in 1559. A 1658 edition, in English, of this work[60] includes the chapter heading: "Of the Helmets of Children newly born, or of the thin and soft caul, wherewith the face is covered as with a vizard, or covering, when they come first into the world." He quotes "old Wives . . . who do but dote, and know not what they say" to the effect that a black caul presages accidents, misfortunes, and haunting by evil spirits unless the caul "be broken and given in drink, which against my will many have done to the great hurt of the child." But if the caul is red or clings to the crown of the head, the child is expected later to achieve great success.[60] Such interpretations were examples of *amniomancy,* the practice of foretelling the future by inspection of the caul.[57, 60] If the caul were "white" (i.e., colorless) or red, it would bring good fortune; if black or lead-colored, the child would be unlucky.[57, 60, 88] In Herzegovinia, a part of Jugoslavia, it was thought that a baby born with a black caul would grow up to become a witch or sorcerer unless, on the first night after the birth, a woman carried the caul to the roof top and announced that "A child was born at our house in a bloody shirt" [*Hemde*]. Elsewhere in Jugoslavia the midwife carried the newborn baby itself to the threshold and announced three times that a real baby had been born, and not a witch or sorcerer.[60] Whether these procedures were regarded as charms or simply as attempts to prevent superstitious rumors among the neighbors is not reported.

Reference to the caul as a "shirt" (see above) illustrates the curious terminology which was developed for the membrane in various countries. Andrews[3] and others have called attention to the fact that semantic changes often involve a shift in meaning from the general to the specific. Certainly

several terms which originally identified the entire amnion came later to signify only the *caul*, e.g., *membrana agnina* and *amiculum* (Latin), *coëffe* and *coiffe* (French) and *Schafhäutlein* and *Kinderbälglein* (German). The word *caul* itself, on the other hand, was sometimes used as a synonym of *amnion*. (*Caul*, in addition, could mean a net, the web of a spider, the base of a wig, a woman's cap, and any of several anatomical investing layers.)[71]

One finds further that the same ideas suggested themselves repeatedly in different lands in the fanciful names appropriated to identify the caul. Examples are: *membrana agnina* (Latin, lamb's skin), *membrane agnelette* (French, lamb's skin), and *Schafhäutlein* (German, little lamb's skin); *galea* (Latin, helmet) and *Helm* and *Knabenhelm* (German, Dutch, helmet, boy's helmet); *pileus, pileolus* (Latin, a close fitting cap like a skull cap); *calotte* (French, skull cap); *Haube, Häublein, Glückshaube, Wasserhaube, Wehmutter-häublein* (German, cap, little cap, lucky cap, water cap, midwife's cap); *silly hood, silly how, sely how, haly how* (English, Scottish, lucky or holy cap); *coiffe, coëffe* (French, veil), *veil* (English); *involucrum* (Latin, covering, wrapper), *Kinderbälglein* (German, child's covering), *súpot* (Tagalog, bag, paper bag); *amiculum* (Latin, dear friend, cloak), *sigurkufl* (Icelandic, lucky coat or cape). Other terms which imaginatively described the caul include *pellis secundina, indumentum* (Latin, second skin, mask), *mask* (English), and *Muttergottestüchlein* and *Kindesnetzlein* (German, Virgin's handkerchief, child's little net).[1, 8, 9, 20, 28, 87, 88, 42, 44, 46, 50, 51, 60, 61, 70, 71, 74, 76, 83, 89, 101, 105] The resemblance of the caul to a close-fitting shirt or under-garment is suggested repeatedly: *indusium* (Latin, shirt), *camicia della Madonna* (Italian, Virgin's shift), and *Hemd, Hemdlein, Westerhemd*, and *Muttergotteshemdlein* (German, shirt, little shirt, vest-like shirt, Virgin's shift.[44, 50, 51, 89, 101, 108]

As would be expected, literary references to the caul are not uncommon. The Oxford Dictionary gives examples dated as early as 1547.[71] *Kell*, a variant of the term, had appeared in print by 1530. Subsequently, the caul was alluded to in English plays,[82, 71] poetry,[40] and elsewhere, including the familiar introduction ("I was born with a caul. . . .") in *David Copperfield*.[83]

Because of the pronunciation and meaning of *cowl*, it has been suggested that this word is cognate with *caul* and the related Scottish *kell*.[34, 71, 98] A fancied similarity of the monk's cowl to the caul is also said to be the basis of the belief that a child born with a caul was destined by Providence for the monastery or convent.[39, 108] In Austria it was expected that a boy would become an archbishop if he carried his caul in his clothes.[50] *Notes and Queries*, that extraordinary repository of antiquarian and other information, offers a quotation from a British newspaper, the Leeds *Mercury*, for 14 September 1889.[70] A laborer's wife bore a son on whose head was a caul. "The veil was placed on one side, and no notice was taken of it until some hours after the child's birth. When examined, however, it was found that the words 'British and Foreign Bible Society' were deeply impressed upon

the veil. When this discovery was made the greatest excitement prevailed in the neighbourhood, some of the women declaring that nothing short of a miracle had been enacted. The doctor, who inquired into the matter, however, soon explained the affair. The veil, whilst in a pliable condition, had been placed upon a Bible, on the cover of which the words 'British and Foreign Bible Society' were deeply indented. The words were in this way transferred to the veil; but some of the inhabitants still ascribe the affair to supernatural influence. . . ."

Elsewhere in England it was believed that, for as long as he kept his caul, the individual born with it would be a wanderer.[88] In Iceland, in the islands near New Guinea, in the Kentucky mountains, and elsewhere one born with a caul was thought to be clairvoyant.[9, 36, 40, 57, 96] The Dutch and the Negroes of North America and the West Indies believed that he could see and talk with ghosts.[7, 41, 47, 57] The caul was also a protection against sorcery, evil spirits, and demons.[9, 57] In Germany the owner carried his caul when he was conscripted, with the hope that this charm would speed his release from military service.[78] Finally, the appearance of the caul gave an indication of the present health of the owner, even if he were absent; a dry, white, crisp caul meant good health, while a damp, black, limp caul betokened illness.[47, 57, 98]

It was regarded as important that the caul either be preserved or disposed of carefully at childbirth. The basis for this idea was very likely the widespread belief that in the caul resided the child's guardian spirit or "life token."[9, 41, 47, 50, 57, 78] In Iceland the caul was buried by the midwife under the threshold over which the mother would later pass.[41, 50] Hastings[47] suggests that this may have been done so that if the child died, its spirit could return to the mother and be born again. In Belgium it was thought that the child would be lucky if the caul were buried in a field, unlucky if the membrane were burned or thrown on the trash heap.[41, 50, 78, 107] The owner would have great misfortune[47] or even die if the caul were lost[98] or torn.[57]

There is a curious report that when a child was christened, the caul was secretly baptized.[50] It was frequently worn in the clothes or carried in an amulet as a protective agent.[41, 47, 50, 78, 80, 108] The caul might even be eaten by the owner in an egg dish, according to Hovorka and Kronfeld.[80] In the islands near New Guinea, a child washed with water in which the caul had been steeped and later fed the powdered caul would be freed of the power of clairvoyance,[36] which evidently was not always regarded as an asset. On the other hand, in Pomerania it was believed that if the caul were burned to a powder and then given to the baby with its milk, the child would become a vampire.[103]

In Germany and elsewhere in Europe the midwife sometimes stole the caul, according to report, to give it to another child,[50, 107] perhaps her own,[78] or to sell it to a witch.[34, 43] The latter were reported to prize cauls highly as potent aids in their evil doing. Vesalius[101] makes passing reference in the

Fabrica to preservation of the caul by ignorant people of some races, adding that both the midwives and the "secret philosophers" greatly coveted this membrane. It is quite clear, however, that Vesalius spurned the superstition.

The *Journal de L'Estoile pour le Regne de Henri IV*,[58] a colorful account of daily events in Paris in the sixteenth century, tells how two priests who were also practitioners of witchcraft battled for a caul in a church on 21 October 1596. It seems that one, *le sorcier,* forgot to take his caul with him when he left the altar. The other, finding the caul, refused to return it, whereupon a noisy quarrel followed *avec grande scandale de tout le peuple.* The finder of the caul succeeded in retaining it, but was promptly accused of sorcery and thrown into prison. He subsequently escaped with the help of his friends and proceeded to take his revenge on his colleague. That possession of a caul was thought to be worth such a scandalous episode clearly illustrates the strength of the superstition at that time.

Twenty-one years later Louise Bourgeois, famed midwife to the court of France, in a textbook of obstetrics (intended for her daughter), spoke her mind on the ethics of midwifery.[18, 40] Mme. Bourgeois issued a specific warning about the caul (page 206): *"Ne retenes jamais la membrane amnios (dit la coiffe de l'enfant, de laquelle aucuns enfans viennent couuerts la teste & les epaules) d'autant que les sourciers s'en seruent"*—that is, never preserve the caul, as it may be used by sorcerers. One such use is suggested in Burton's *Anatomy of Melancholy*[20]: a list of ingredients for love potions or "philtres" includes dust of a dove's heart, rope with which a man was hanged, tongues of vipers, and "the cloak with which infants are wrapped when they are born" (page 498). Brissaud[19] tells how girls would ask the women assisting in a delivery to save the caul, "believing that if this powder were given to a man, he would at once fall in love with a maiden." In Germany a young man might carry his caul with him to aid him in winning the affection of a girl.[78] In the southern Slavic countries a girl wore her caul as a love charm; if she then touched the skin of a man, he would instantly be attracted to her.[50] Lean (1903)[58] reports that in Scotland there is the expression, *rowed in his mother's sark-tail,* that is, wrapped in his mother's shift, as well as the belief that an adult who as a baby had been so protected would be successful in his love affairs. It is suggested that all this related to the superstition about the caul as a love charm.

According to *The Golden Bough,* on the island of Timor the owner of a caul may use it as a charm to save a failing rice crop.[56]

The caul was frequently believed to confer eloquence on its owner. Several sources report that St. John Chrysostom (347?-407 A.D.), one of the Fathers of the Church, inveighed against the superstitious use of the caul as an aid to persuasive speech, but a rather extensive search of his writings has not disclosed the passage. However, Theodorus Balsamo,[8, 66] a twelfth century canonist who likewise criticized employment of the caul as

a charm, tells of a prefect who "was arrested while evidently carrying in his bosom a caul [*indumentum*] of a newborn infant, and said this had been given to him by a certain woman for the purpose of turning away and stopping up the mouths of those who tried to speak against him. Whence he was the subject of condemnation." Aelius Lampridius[33, 35, 62, 69, 86] tells how the Roman midwives made off with cauls, which could be sold to credulous lawyers. The latter believed that possession of the membrane would help them to plead their cases successfully. The same superstition appeared also in Iceland, Denmark, England, and elsewhere in Europe.[9, 50, 107]

The caul was considered valuable as a remedy, both general and specific. From Norse mythology came the belief that if the caul had been dedicated to the Norns, giant goddesses who held men's fates in their hands, childbirth would be easy.[79] In Denmark it was thought that if a woman crept under a foal's caul stretched on sticks she would have a painless labor, but as a penalty her sons would become were-wolves and her daughters nighthags.[68] Ambroise Paré refers[73] to the belief that birth with a caul confers happiness. He adds the clinical suggestion that in a difficult birth the amnion would always be stripped away from the infant, just as "the Snake or Adder when she should cast her skin thereby to renew her skin, creepeth through some strait or narrow passage."[52] Mauriceau, in his textbook of obstetrics,[65] rejects the superstition and dryly remarks that children with cauls "may be said to be fortunate, for having been born so easily; and the Mother also for having been so speedily delivered; for the difficult Labours, Children are never born with such Caps, because being tormented and pressed in the Passage, these Membranes are broken and remain there" (cf. Paré's comment, above).

Sir Thomas Browne[30] recognized the caul superstitions of his period: "Great conceits are raised of the involution or membranous covering, commonly called the Silly-how, that sometimes is found upon the heads of children upon their birth; and is therefore preserved with great care, not only as medical in diseases, but effectual in success, concerning the Infant and others; which is surely no more than a continued superstition." According to Thomas Bartholin,[10, 13] the caul could be made into a medicine and hand lotion for mother and child. He adds a matter-of-fact commentary on the state of professional ethics: if the midwife preparing such a concoction used a substitute for the caul, she could be arrested!

Hovorka and Kronfeld[50] have described an old remedy for malaria. Its remarkable ingredients include snails, egg white, laurel berries, powder from a burned piece of shirt, rust from a coffin nail, a pinch of burned and powdered human bones, *and* a powdered caul. Unfortunately, the rationale for this masterpiece is not stated. In Dalmatia, the caul was placed under the owner's head when he lay on his death bed so that his passing would be easy.[80]

If the caul could bring good health, it could also ward off disaster and danger. There is a relatively recent report of an English farmer who ascribed his own good health and that of his two sons, then fighting in World War I, to the fact that each of them carried the caul of a lamb born in the farmer's flock.[102] This would seem to be simply an extension of the idea of the protective influence of the human caul. (One recalls that both the human amnion and the caul proper were sometimes described in terms comparing them to lambskin.)

Coal miners carried cauls to prevent the explosion of fire damp.[19]

One of the most widespread of all the caul superstitions was that it would protect against drowning.[19, 58, 84] Perhaps the most impressive support for this idea is a perfectly serious statement quoted in *Notes and Queries*[84] regarding a baby born with a caul so effective "that when his mother tried to bathe him he sat on the surface of the water, and if forced down, came up again like a cork." McKenzie,[66] Fairfax-Blakeborough,[34] and others have suggested that the belief in the caul's ability to preserve its owner from drowning is assumed from the membrane's investment of the fetus while it is immersed in the amniotic fluid. Naturally the caul had a particular appeal to sailors. Thomas Hood, the humorous poet, told[69] of a "jolly mariner" who defied a storm because "in his pouch, confidingly, He wore a baby's caul." However, the charm did not prevent disaster in a great storm:

> "Heaven never heard his cry, nor did
> The ocean heed his caul."

Cauls were formerly offered for sale near the London and Liverpool docks,[4] and advertisements for this commodity appeared in British newspapers until at least the First World War:[91] in the *Daily Advertiser* for July 1790, in the *London Times* for 20 February 1813 and 8 May 1848, in the *Bristol Times and Mirror* for 30 September 1874 ("TO SEA CAPTAINS: For sale, a Child's Caul in perfect condition. £5."), in the *Globe* for 24 July 1903 ("Large Male Caul for sale; no reasonable offer refused."), etc.[48, 55, 64]

The prices asked for cauls, while perhaps little influenced by the supply, which must have been reasonably constant, certainly fluctuated with demand. The latter seems to have been regulated largely by the degree of hazard of the sailor's life and his consequent concern for his safety. Thus, one reads that in 1779 as much as 400 marks was paid for a caul. By 1799 the price paid by British sailors reached 30 guineas. (These were the days of Lord Nelson and great sea battles.) By 1815 the price was down to 12 guineas, and by 1848 a caul was advertised for sale in *The London Times* for 6 guineas. The advertisement states that the caul, "for which fifteen pounds was originally paid, was afloat with its late owner thirty years in all perils of a seaman's life, and the owner died at last in the place of his birth." In 1874 a caul was offered "TO SEA CAPTAINS" for £5. In

1895 a newspaper advertised a price of £5 "or offers"; a week later another advertisement (apparently by another owner) had cut the price to £1 "or offers." A still lower point was reached in the last years before the First World War, when cauls could be purchased for a few shillings. Superstition, however, had not died; when later the deadly submarine campaign was taking its toll, worried sailors and their friends paid as much as £3 to £5 for the protective membrane.[23, 24, 64, 68, 91, 103]

The longevity of some superstitions is surprising until one realizes that the basic phenomenon is often the desire to believe. Fear of the future, worry over personal safety, uncertainty as to a favorable outcome—all these are facets of man's terror of the unknown. One battles the terror in various ways. The educated man is deprived by his very education of most of his ability to believe blindly, and sophistication usually cloaks the lingering remnants of his credulity. (We have seen that belief in the protective value of the caul was rejected by the physician.) Hence he is likely to seek through a reasoned approach some relief from his fear of the future. On the other hand, every age, every culture has provided an abundance of superstition for those who would accept it. The good health, success, and safety offered by the caul beliefs are tempting enough. It is small wonder that these superstitions have persisted. It is likely that they will continue to survive.

Mention must now be made of a group of related beliefs. Witkowski[108] says that the *hippomane,* a potion of the ancients, contained as one of its ingredients the fleshy membrane covering the head of a foal at birth. Classical writers applied the term *hippomanes* to a fleshy mass said to be found on the head of a newborn foal. It appears that the hippomanes and the equine equivalent of the caul, although different structures, have been confused. Both are unusual remnants of the fetal membranes, and the relevant superstitions have much in common.

In ancient times and down through the Middle Ages, *hippomanes* (Latin, Greek; French, *hippomane;* German, *Fullengift, Fohlenmilz, Fohlenbrot,* etc.) might signify a poison, a love potion or ingredient thereof, a plant which produces estrus in the mare, estrus itself, the estrous secretion, or particularly a bit of dark flesh said to lie on the forehead of the newborn foal.[6, 26, 27, 56, 71, 72, 82, 94, 97, 100, 108] According to recent interpretations the hippomanes is the result of a local accumulation of the secretion from the glands of the pregnant uterus. The enlarging mass causes in the horse the progressive invagination of the overlying area of chorio-allantoic membrane into the allantoic cavity or, in the lemur, into the yolk sac.[45, 94] When fully formed the hippomanes consists of a small round or elongate mass of viscous material invested with chorio-allantoic or chorion-yolk sac membrane; the membrane also provides the pedicle connecting the structure to the inner surface of the allantois or yolk sac proper. Thieke states[94] that the

hippomanes may even become detached and lie free in the allantoic cavity between the embryo and its membranes.

One can imagine that a foal might be born with either a folded amniotic fragment (the equivalent of the human caul) or a true chorio-allantoic hippomanes adhering to its head; presumably such an occurrence inspired the inaccurate classical concept. Aristotle describes[6] the growth as black, round, and smaller than a dried fig. The mare, he says, promptly bites off and swallows the hippomanes; if someone steals it (as a love charm), its smell makes the mare frantic. The mass was highly prized as the ingredient of potions. Vergil tells[99] how Dido, desperate because Aeneas was going to leave her, asked a witch to cast magic spells. The witch invoked the deities of darkness, and scattered herbs and water from Lake Avernus:

> *quaeritur et nascentis equi de fronte revulsus*
> *et matri praereptus amor.*

—"and the hippomanes [*amor*, literally, the love], snatched from the forehead of a horse at birth, forestalling the mother, is sought."

In the first century A.D., Lucan, discussing love potions, mentions[62] the hippomanes as a powerful agent. Juvenal and Suetonius both tell[56, 93] how the Empress Caesonia prepared a love potion from the hippomanes for her husband Caligula; the philtre, however, drove him mad.

Pliny, writing in the first century,[77] Aelian[2] and Solinus[87] in the third century, and Photius in the ninth century[75] followed Aristotle closely in their descriptions of the hippomanes. Aelian added that the mare's rage when the *caruncula* is stolen is due to her jealousy of the sorcerers who would make use of the mass in potions for human beings.

After Europe emerged from the Dark Ages, references to the hippomanes again appeared. Porta mentioned it in his *Magiae naturalis*[81] in connection with love potions, as did Wier in a chapter entitled *De Veneficis*.[104] (It is noteworthy that *Veneficis* signified both sorcerers and poisoners, just as *veneficium* meant both witchcraft and poisoning.) Ruini's fascinating book on the horse[84] told, as Thieke has pointed out,[94] of a structure which certainly must be the hippomanes, although Ruini did not give it a name. In a horn of the uterus of the pregnant mare, he says, there has been found an unattached mass which is usually lead-colored, egg-shaped, "and about a half-finger thick if the mare has not carried it for long." Ruini believed that this mass was the poisonous residue of the semen. Clauder remarked[25] that "What the hippomanes actually is, and what its proper use may be, has exercised the wits of physicians and philosophers in many ages. . . ." He rejected the superstitions, including the idea that prompt removal of the mass ensured that the foal would become a swift racer and the belief of a colleague that the administration of 10 grains of dry hippomanes would protect a child against epilepsy.

Bayle's great dictionary discussed[13] the hippomanes superstitions in some detail, but did not imply their current acceptance. A few years later, in

1755 and 1756, Daubenton[28, 29] published what appear to be the first attempts to investigate the hippomanes in scientific fashion. As a comparative anatomist particularly interested in the horse, he dissected many pregnant mares and repeatedly found gelatinous masses in the uterus between, he says, the allantois and the amnion. He concluded: *"Cette expérience prouve clairement que l'hippomanes est un sédiment de la liqueur contenue entre l'allantoïde & l'amnios. . . ."* He also suggested[28] that very rarely a colt might be born with a hippomanes bound to its forehead by an amniotic remnant or *calotte*, like the infants *"qui naissent coëffés, selon l'expression vulgaire. . . ."* He reported[29] finding the hippomanes of the donkey, cow, deer, goat, and ewe.

Buffon in his *Histoire naturelle*[31] and Bourgelat in a volume on veterinary medicine[17] both discussed the hippomanes with considerable understanding. Sind, in another veterinary text,[65] supported the incorrect idea that the hippomanes is a piece of fetal membrane. Nineteenth century works on comparative anatomy described the structure less erroneously and in more detail.[16, 24] It remained for Thieke[64] and Hamlett,[45] however, to dispel misunderstanding of the phenomenon by explaining it. The term *hippomanes*, horse madness, itself survives, testimony to a superstition of the days when biology was born.

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A STUDY OF 300 CONSECUTIVE CASES OF ABDOMINAL WOUND CLOSURE WITH ALLOY STAINLESS WIRE SUTURES

JOHN M. FREIHEIT* AND PAUL R. DE VILLERS

For the past eight years, following closely on the advent of early ambulation of surgical patients, the use of alloy steel wire sutures in the closure of abdominal wounds has been the incentive for many reports and much clinical discussion. The pioneer work in the use of metallic suture was reported by Babcock,[1] Jones,[5] Kaufman,[6] and others.

This article is a clinical summary of 300 consecutive cases of routine closure with alloy steel wire offering evidence which we feel indicates its superiority over other forms of suture material in application to present-day needs and policies for the protection of the patient from so-called unavoidable complications.

Twenty-five years ago the senior author began his surgical training under men who were disciples of Halsted. For years he advocated silk until the disadvantages of the material became apparent and made him reconsider its usage. These have been summarized by DeCoursy[8] as follows:

(i) Silk is nonabsorbable, therefore only interrupted sutures can be used.

(ii) When infection is present, a persistent sinus may be formed. The silk may be extruded any time up to two years after operation. If the silk is not extruded, tedious dissection may be necessary.

(iii) With large stay sutures especially, the movement of tissue fluids is promoted by the capillarity of the strand and bacteria may thus be carried throughout the whole wound.

(iv) Deeply buried silk may cause a rare tumor (Schloffer's).

(v) Ends of silk sutures irritating bowel may cause ulceration.

We therefore turned to catgut of the chromicized type—being contemporary with Howes[4]—since an absorbable suture does not cause sinuses, foreign body reactions, tumors, or permanent ulceration. The thing that always impressed us about catgut was that it had a period of weakness due to enzymatic reaction in the tissues; in the presence of infection this reaction is especially marked, and a wound closed with catgut is weakest between the fourth and eighth days after operation, causing a considerable decrease in tensile strength at a time when the tissues themselves have not had sufficient time for fibroplastic repair.

* Assistant in Obstetrics and Gynecology (Assistant Resident and Resident, New Haven Hospital) 1928-30.

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Despite the disadvantages of catgut, so long as patients were kept in bed for eight to ten days postoperatively we had wonderful results with its use in wound closure. However, in the course of healing, wound infection was unavoidable in a considerable percentage of cases, and stitch abscesses were not uncommon. Patients reacted to them quite unfavorably, and we know that in the case of two Caesarean operations patients sought another obstetrician who "did not get stitch abscesses."

However, the senior author dreaded early ambulation when it came and was probably the last to succumb to its popularity in this area. By minimizing hemorrhage with absolutely dry wounds, avoiding bunching of tissues and subsequent necrosis, using chemotherapy and its successor antibiotics prophylactically, realizing that catgut was "improved," and paying more attention to protein deficiencies and vitamin deficiencies, he gradually came around to early ambulation—not so much by choice, probably, as through coercion and the rising economic pressure of hospitalization on the patient. Yet his distrust of early ambulation with catgut plagued him. Occasional infection and stitch abscess persisted and finally early in 1947

FIG. 1A. Pathological report 5B71. "Sections of the scar tissue show the tract of a steel suture about which a number of mononuclear phagocytic cells have fused representing a mild foreign body reaction. Giant cells are absent. Inflammatory reaction is relatively scant."

This was at a second Caesarean section. With the third section the same patient, a Negress, had some keloid formation in her scar but the pathological report was as follows:

FIG. 1B. Pathological report 5-6082. "Sections of the scar show the track of a suture in which there is only the mildest degree of foreign body reaction. The scar is hypertrophic after the fashion of a keloid."

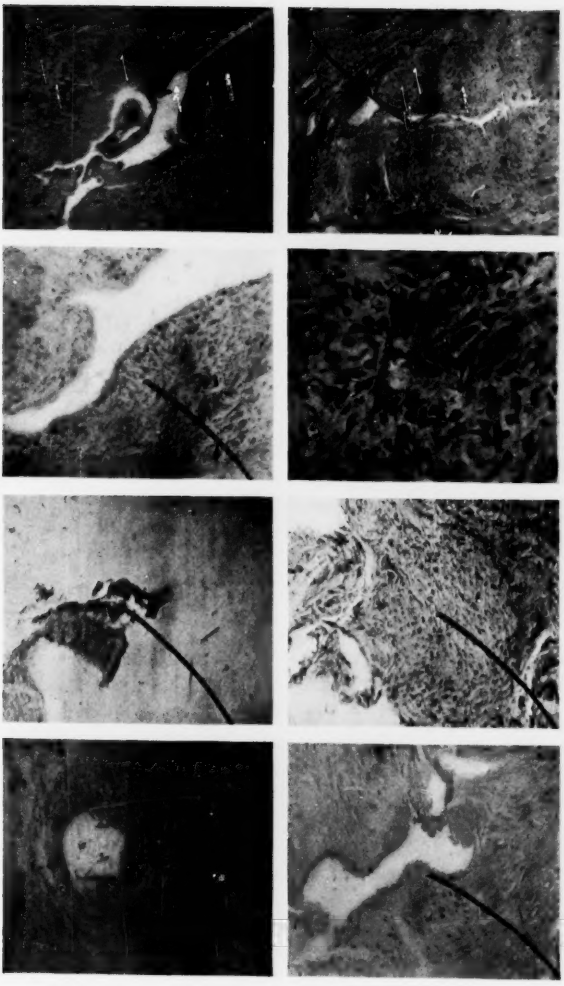
FIGS. 2A (*c.* x75) and 2B (*c.* x250). One-year postoperative. Pathological report 52820. Sections of the fascial tissue failed to include a well-defined cross section of the path of the wire. Considerable scar tissue formation is present but no foreign body reaction can be detected.

FIG. 3. Two years postoperative. Pathological report 50249. Sections of the fascia show no tissue changes which can be attributable to suture material. In fact, although the wire was removed from the specimen before fixation, the track of the wire suture could not be identified in several cross sections.

FIG. 4. H3586. One year postoperative. Pathological report 51929. Sections show fibrous tissue traversed by a narrow canal from which a metal suture has been removed. There is a moderate foreign body reaction about the suture with some infiltration by round cells. No giant cells can be identified. The inflammatory reaction is relatively slight.

FIG. 5. H62242. Two years postoperative. Pathological report 55859. A section of fascial tissue shows the path of a wire suture in which there is minimal inflammatory or foreign body reaction.

FIG. 6. H46941. Three years postoperative. Pathological report 47103. Sections of fascial tissue submitted for study with wire imbedded shows relatively slight foreign body reaction and only small areas of perivascular round cell infiltration. Foreign body cells are quite few in number. The foreign body reaction is decidedly less than that usually encountered with silk or catgut suture material.



(From left to right) Figs. 1a, 1b, 2a, 2b, 3, 4, 5, and 6.

a patient who had had a totally uncomplicated hysterectomy for fibroids had an eventration a few hours after her skin clips had been removed on the seventh day. Wound resuture kept the patient in the hospital for another ten days and four months later she had to spend another ten days in the hospital for repair of the usual ventral hernia that follows so many of these accidents. The younger surgeons returning from overseas duty were using cotton and linen in wound closure. It did not take long to decide that the disadvantages of this type of suture were the same as and even more pronounced than those of silk, especially in the presence of infection.

In the fall of 1947 John D. Booth[2] published a paper in the *Connecticut State Medical Journal* on the use of metallic suture with early ambulation, especially with reference to the prevention of thrombophlebitis. We had never seen in our patients any decrease in thrombophlebitis because

TABLE 1
OPERATIONS PERFORMED

Panhysterectomies	114
Supravaginal hysterectomies	14
Caesarean sections	99
Appendectomies	13
Cholecystectomies	14
Gastric resection	1
Wertheim operations	1
Bowel resections	2
Combined panhysterectomy and cholecystectomy	1
Miscellaneous	41
Total	300

of early ambulation. The problem of early ambulation has arisen mainly because of economic considerations. The occasional patient who suffers from thrombophlebitis has usually been discharged from the hospital on the sixth or seventh day, and develops the thrombophlebitis around the tenth day. Since one does not dare to jostle her around in a car or ambulance in bringing her back to the hospital, she is often treated at home. The thing that impressed us most in Dr. Booth's paper was his description of the qualities of the wire alloy as developed by Wayne Babcock[3]—high tensile strength, malleability, minimum tissue reaction, rapid wound healing, no complications in the presence of infection—factors which were lacking in catgut.

In 1948 after a visit to Dr. Plass's Clinic in Iowa City the senior author became interested in the stainless steel alloy for abdominal wound closure that both the surgical and gynecological groups were using there.*

* This is a product of the Alloy Metal Wire Company, East Orange, New Jersey.

Its high tensile strength is ideal for allowing early ambulation; its low thermal conductivity will not cause irritation if diathermy or other high frequency currents are used for therapeutic purposes at any time after operation; its ductility causes minimal tissue irritation by free ends, especially if they are cut short on square knots. It is used in two sizes: (i) heavy or .009 inch, single strand, containing 4,382 feet to the pound, and (ii) fine or .005 inch, single strand, containing 14,876 feet to the pound. Compared

TABLE 2

NUMBER OF PATIENTS IN RESPECT TO DAYS AND EARLY AMBULATION

Day	*Dangle*	*Chair and blanket*	*Skin sutures out*	*Discharged*
1	213	16		
2	60	188		
3	20	58		
4	6	21		
5	0	10		14
6	1	1	7	12
7		5	151	62
8		1	120	131
9			20	48
10			2	17
11				7
12–18				9

100% Of wounds were healed in ten days without incisional complications.
91% Dangled the first or second day postoperatively.
87% Were sitting up by the third day and feeling well.
93% Of wounds were healed and the patients discharged by the seventh and eighth day without any complications.
73% Were discharged by the eighth day.
22% Were discharged the ninth and tenth days.
95% Of the 300 patients in this series were home by the tenth day with healed wounds. The remaining 5%, although healed, were confined for an additional five or six days because of pyelitis or cystitis, or constitutional weakness which required additional rest.

to catgut with its intricate preparation and sterilization requirements, the cost is ridiculously low. To prepare the wire for surgical use it is wound around an eighteen- to twenty-inch board in strands of 24 turns and cut at both ends of the board along a groove placed one inch from either end on alternate sides. The forty-eight sutures obtained are then placed in glass tubing eighteen inches long with a one-fourth- to one-half-inch bore. These tubes have been previously sealed at one end and about one inch of the wires is left protruding through the open ends. Suitably wrapped, the tubes are autoclaved—one of each size per package. Glass tubing is preferred because the stainless steel softens and loses its tensile strength through repeated boiling or autoclaving in metal tubes.

Catgut is used routinely in the peritoneal cavity. Metal is not employed there except for a few special purposes which will be described later. A

zero chromic continuous suture is used to close the peritoneum although Robinson[7] employs continuous metal for this also. Six to seven of the heavy .009-inch wires are placed as figure-of-eight sutures in the anterior rectus fascia for the midline superpubic wound, care being taken not to include muscle tissue in the tie, and finally ten to fourteen of the fine .005-inch wires are used in mattress fashion to close the skin.

It does not matter whether one or twenty sutures are expended from the prepared tubes at each operation; the remaining sutures can be repackaged and re-autoclaved for another operation with minimal loss. If too many catgut sutures are opened for an operation, they represent a total waste.

TABLE 3
THE RESULTS AS REGARDS COMPLICATIONS

Number of Patients	300		
Total wound complications—external	0		
Total wound complications—internal	2		
Wound infections	0		
Eventrations	0		
Incisional hernia	0		
Painful wounds	0		
Serum pockets	0		
Sinus tracts	0		
	1st day	*2nd day*	*3rd day*
Average temperature postoperatively	99.9	100.0	99.5

The deep fascial sutures are permanent. The skin sutures can be removed in six to seven days postoperatively. In exceptionally stout women and in any patient in whom infection is anticipated it is wise to place two or three retention sutures of .009-inch steel in the usual fashion through skin, and superficial and anterior rectus fascia; otherwise retention sutures are not used. In this series we employed retention sutures in only two patients with extremely obese abdomens.

Ordinarily the fascial sutures are not tied tightly but are merely adjusted to approximate tissues. The wire knots must always be tied square and the ends cut upon the knot leaving nothing to cause tissue irritation with motion. Once in a while a patient comes to the office several months postoperatively complaining of incisional pain after heavy lifting or straining. One treatment with diathermy relaxes the spasm completely.

In gynecological operations care is taken to close all enteroceles of the cul-de-sac. Proper repair of this condition makes all the difference between a successful or an unsuccessful operation, just as proper evaluation of sphincter control makes all the difference between a successful and an unsuccessful operation for cystocele. The use of the wire in the repair from

above of enteroceles is not advisable. Here, fine braided silk in purse string concentric layers is preferable. Stainless steel .005 in this procedure was tried twice; both patients developed abdominal distention and paralytic ileus—one without any additional complication and the other with a hematoma which had to be explored from above and from which non-infected clots were removed before recovery was satisfactory.

The only place we find stainless steel useful in the abdomen is where it can be extraperitonealized. Gynecologically, this is possible in two instances; in suspension of the uterus, especially in Baldy-Webster's type, and in suspending the vaginal stump in panhysterectomies. It is quite useless to believe that suspending a stump with catgut guarantees that in every instance it will hold until the tissues heal sufficiently to give adequate support. This is definitely a complication in early ambulation and has altogether destroyed our faith in catgut for fixation purposes. We have re-operated upon two patients for what appeared to be ovarian lesions appearing from six months to a year after the first operation. Both of these had had stump suspensions with catgut and early ambulation. It was found in both that the catgut fixation suture had parted and that the round ligaments were widely separated as if no suspension had been attempted. In each instance the parovarian space was the seat of an organized small hematoma, one of which partially blocked the right ureter. It is believed that the separation and subsequent hemorrhage occurred as a result of early ambulation; it is felt that suspension sutures, in order to hold securely, should be permanent sutures. In the last year we have been using in all hysterectomies one stitch with stainless steel .009 to fix the round ligaments to the vaginal stump, this being covered with the bladder peritoneum. The ends are cut short as in a fascial closure. A high suspension of a vaginal stump causes no discomfort and actually helps to elongate the vagina postoperatively. It fails to do this with catgut sutures that part under the strain of early ambulation. Baldy-Webster suspensions will be found to hold in all instances when the first suture for posterior fixation of the round ligaments is .009 stainless steel. This can be covered by suturing the peritoneum with fine catgut in order to protect the bowel.

We have ceased to use drains in the lower abdomen or through the vaginal stump except in the presence of frank pus, either with acute appendicitis or with chronic pelvic abscesses. Gall bladder wounds are drained, preferably through a stab wound in the right flank. All patients postoperatively receive penicillin and streptomycin combined, once daily for three or four days. Previously to 1950 we used penicillin and sulfonamides combined, parenterally and orally respectively.

It is primarily due to antibiotic therapy alone or combined with chemotherapy that we attribute the low postoperative temperature in this series. The stainless steel itself receives credit only for the tensile strength of the wounds in the early postoperative period when early ambulation calls for

more strain on sutures; wire gives this added strength when it is most needed. It also produces less tissue reaction even in the presence of infection. In many of the abdominal operations here recorded, acute or subacute infection would have contaminated catgut and this would have resulted in wound infection, occasional rupture, and numerous stitch abscesses as was our previous experience. These complications were formerly classified as "operative risk" and although eventrations were rare, when they did occur, they formed the background for it. Here is a suture material that regardless of the risk has not been followed by eventrations and stitch abscesses and has produced an astoundingly rapid wound healing in three hundred consecutive cases without exception. Internal complications will also diminish when we learn the judicious use of wire for support.

In obstetrical cases we have been suturing episiotomies with catgut in the vaginal mucosa, imbedding one or two chromic sutures in the perineal muscles and the levator ani and closing the skin margins with subcuticular .009 stainless steel. There is less tissue swelling with this type of closure and the patient complains of little discomfort. Even with first day ambulation, there is no danger of wound breakdown. The suture is removed on the fifth day and the patient discharged at that time. We have not included any perineal closures in this series but they make up an additional group who, with added safety in their recovery, have had no infections in an area prone to it.

Vaginal operations adjacent to the neck of the bladder, since they are all extraperitoneal, provide further opportunities for the judicious use of wire alloy. In the Marshall-Marchetti operation for stress incontinence we use .009 stainless steel in the pubic periosteum as a permanent suture. None of these have failed and there have been no infections. This operation, however, should not be relied upon without an adequate repair of the anterior wall from below. This in size .005 for high sutures in the colpocleisis operation has also given good results.

Repeat Caesarean sections and ectopic pregnancies following previous operations have given opportunity for study of the permanent sutures in the fascia as long as three years following their use. The pathological reports on this material have been consistent in that reaction to the wire sutures is minimal (Figs. 1 to 6).

Summary

A consecutive series of 300 cases of abdominal wall closure with alloy stainless steel has been presented together with a discussion on the use of the material for various lower abdominal procedures. No wound complications occurred in the entire series and this includes infections, eventrations, hernia, painful wounds, serum pockets, or sinus tracts. In the prevention of painful wounds the sutures should not include the rectus muscle when approximating the fascia. Faith in the tensile properties of this material

should not lead one to ignore ordinary surgical principles of drainage and tension sutures when required. The use of steel is not a short-cut to closure, but if anything requires more careful attention to detail, the slight additional time is more than offset by results. No "broken sutures" have been encountered with this method. Recognizing that this is a non-absorbable suture in deep tissues, minimal amounts of it should be left consistent with the procurement of a sturdy anterior fascial wall. In the average wound 10-15 cm. in length this requires some six to eight figure-of-eight sutures in the fascia.

The wire is not recommended for the internal closure of enteroceles. It is, however, recommended for the suspension of the vaginal and cervical stumps, and for round ligament suspension and ventral fixation. Anywhere the suture material is in approximation with serosa, it should be covered by drawing a portion of the peritoneal surface over it with a mattress suture of plain or chromic catgut. The wire suture may be used for Marshall-Marchetti suspensions of the neck of the bladder and peri-urethral tissues. Vaginally it can be employed for the colpocleisis operation and in obstetrics it prevents painful episiotomy closures as a subcutaneous stitch in the perineal skin, after approximation of the rest of the wound with fine catgut. It can be removed, here, in five days with perfect closure.

Altogether our experience with this wire has been so gratifying that we feel it should remain the routine closing method in order to overcome any dangers that might ensue from early ambulation.* With this method of abdominal wound closure, where a satisfactory home nursing service is available, more than 80% of abdominal operations of the type here described could be discharged from the hospital by the fourth or fifth day postoperative, and the sutures removed between the eighth and tenth days in the surgeon's office.

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* This series has at the time of printing risen to over 500 consecutive cases with continued satisfactory results. We have found in the last 200 that have not been reported two instances showing more than the usual foreign body reaction around wire present in the tissues for over two years. Neither of these, however, gave any evidence of discomfort to the patients, and both were in infected wounds at the time of the first operative procedure.

CEPHALIC VERSION IN BREECH PRESENTATION

IRVING FRIEDMAN*

INTRODUCTION

Breech delivery has a usual incidence of about 4 per cent, and virtually all obstetricians agree that the infantile mortality in these cases exceeds that of vertex delivery and the danger of serious injury to the baby is increased. The fetal hazard may be reduced, at some added risk to the mother, by extending the indications for Cesarian section, particularly in elderly primigravid patients, or those with borderline cephalopelvic disproportion. For infants delivered vaginally, the skillful conduct of labor is of preëminent importance. There are, indeed, a few reports, such as those of Gustafson,[4] Hansen,[5] and Arnot and Nelson,[1] of breech deliveries done with extraordinarily little fetal loss attributable to delivery itself. These excellent results were achieved in private practices and are a tribute to the unusual dexterity and obstetrical judgment of the authors. In a large series of cases from a hospital service, Seeley[16] found that even with expert management the corrected fetal mortality for viable term babies was three or four times greater for breech than for other deliveries. Guyer and Heaton[7] found a four and a half times greater fetal risk inherent in breech delivery. The experience of most others[3-5, 9, 10, 12, 13, 15, 17] seems to range from results approximately as good as these to considerably worse; though it is difficult to compare published reports, since the various authors use different standards for correction of their statistics. The consensus leaves little doubt, however, that survival and well-being of the baby are jeopardized by breech delivery. Management of the patient with breech presentation therefore presents a problem deserving of careful consideration.

Another approach to this problem is to reduce the incidence of breech delivery by the practice of external cephalic version during the latter part of pregnancy. This is advised by the great majority of authors. Seeley[16] found seventeen of twenty-three recommending it. Version is advised, for selected cases at least, by the authors of fourteen of the papers in the bibliography of this report, while the authors of but one[14] consider it of little value; the remainder omitting all discussion of version[6, 15] or reporting briefly some unusual complications.[11, 19] Bartholomew[2] estimated that the frequency of breech deliveries may be reduced 80 to 90 per cent by the routine practice of external version during the last two months of pregnancy, but actual reports hardly approach this measure of success, probably

* Assistant in Obstetrics and Gynecology (Assistant Resident and Resident, New Haven Hospital), 1938-40.

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in part because version was not attempted persistently or practised repeatedly. Olow and Posse[14] are among the few authors of recent papers who consider prophylactic version of no practical value. Sometimes the baby cannot be turned, and successful versions are often followed by spontaneous reversion to the previous position. The procedure is not entirely without risk, moreover, chiefly because of the danger of placental separation, prolapse of the cord, or strangulation of the infant by entanglement or knotting of the cord. The risk is usually slight. Jokela[9] collected from the literature 3,646 successful versions by 28 authors, with a gross fetal mortality of 3.21 per cent. This is much less than the infantile mortality for a comparably assorted series of breech deliveries and indeed is hardly more than the gross fetal loss for vertex delivery. These results are particularly impressive since many of the cases were turned under anesthesia, which greatly increases the hazard. Thus, Wrigley[19] found that among 76 patients who were anesthetized for the purpose of external cephalic version in two London hospitals, the infant was successfully turned in 45, but 10 of these infants died. Except for this appalling report, the gross fetal mortality for the various series of Jokela's collection ranged from 0 to 5 per cent. It therefore appears that external version is a relatively safe procedure in a variety of hands, which is more than can be said for breech delivery. The maternal risk of version is chiefly that inherent in the possibility of placental separation and is small indeed. McCullagh,[11] however, reported four extraordinary instances of compound presentation following external cephalic version, with dystocia terminating in rupture of the uterus and death of the mother in each instance. He attributed this accident to the extended position of the fetal legs and advised against version of frank breeches. These are more difficult to turn in any event, and others too have advised against version of frank breeches, recommending it for other patients.

Material and Purpose of the Present Study

The present study was undertaken to determine whether or not the incidence of breech delivery can be reduced effectively by diligent attempts at external version and to evaluate the dangers of this practice. It is a report of cephalic version in a series of 500 consecutive private patients each delivered between 1949 and 1952 of a single infant weighing 1500 grams or more at birth. External version was attempted in cases of breech presentation, usually after 32 weeks of pregnancy.* If the attempt was unsuccessful, it was later repeated, and spontaneous reversions were turned again, repeatedly if necessary. This was done even in cases subsequently to be delivered by Cesarian section, which are therefore included in this series. Observations were made of the occurrence of spontaneous versions, both cephalic and podalic, in an attempt to evaluate some of the factors influencing fetal position *in utero*.

* Throughout this report, the week of pregnancy is reckoned from the first day of the last menstrual period in women having normal menstrual cycles. Suitable corrections are introduced for women having abnormal cycles.

Technique of external cephalic version. Having emptied her bladder, the patient is postured upon the examining table on her back, with hips slightly elevated, thighs and legs resting in a position of flexion, and arms lying loosely at the sides. With the patient in this position, the gravid uterus tends to move from the pelvis into the abdomen, and the abdominal muscles are relaxed. The abdomen is powdered with talc. Deep, slow, diaphragmatic respiration is advised, and psychic and muscular relaxation are encouraged. Anesthesia is not used.

With the flat of the fingers of both hands, the breech is gently lifted out of the pelvis to as high a level as possible and displaced to one side. An irritable uterus will sometimes be stimulated to contraction, and manipulation should then be interrupted to coincide with intervals of relaxation. If the breech is deeply engaged and cannot be elevated in this manner, an assistant dislodges it by gentle vaginal pressure. Should this fail, the attempt at version should be abandoned for the while, to be tried again at a more opportune time. A few days or a week later, it may be found that the breech can be dislodged with ease, or indeed that the baby has turned spontaneously in the interim. No attempt should be made to initiate version by pressure upon the fetal head if the breech has not been dislodged first. Apart from the fact that such a maneuver is mechanically unsound, and unlikely to succeed, it probably increases the risk of causing placental separation, since the placenta usually occupies the uterine fundus.

When the breech has been raised from the pelvic inlet and displaced aside, it is held with one hand to prevent it from slipping back, while the head is pushed toward the inlet with the other hand. One fetal pole is advanced at a time. Once initiated, the process of version is usually completed most easily by manipulation of the fetal head. When version has been completed, the patient's hips are lowered and the fetal heart sounds checked. The patient is allowed to rest in the supine position for 15 or 30 minutes, during which time the fetal position and heart sounds are checked again. It is usually advised that the fetus be turned back if it exhibits signs of distress, but this situation has not arisen in the author's experience.

The baby is usually turned through the short arc: thus, if the head lies in the right side, the breech is displaced to the left. Failing in this direction, version through the long arc may be attempted. Version need not be restricted to the direction which maintains the posture of flexion, since the baby's active movements will quickly correct any deflection which may occur during manipulation.

Breech position was noted in 94 of the 500 patients at one time or another during the last trimester of pregnancy, an incidence of 19 per cent. In the early part of the series attention was focused upon external version only, and the fetal position was often not recorded until 32 weeks of pregnancy or later. Some breeches which later turned spontaneously were therefore missed. Whenever possible, the fetal position was recorded from 28 weeks to term in the later patients of the series, and among these, breech position was noted at some time in 23 per cent of cases. This is almost certainly a closer approximation to the true incidence and is in better agreement with Vartan's[18] estimate that one woman in four exhibits breech presentation during the latter part of gestation.

External Cephalic Version

External cephalic version was performed upon 60 patients, or 12 per cent of the series. In 43 cases the fetus remained in vertex position after a single version. The other 17 cases reverted to breech position and were

turned again, repeatedly when necessary. Twelve of these were turned twice, four were turned three times, and one was turned four times. Version was done 83 times in all. All infants in this group were in vertex position at the time of delivery.

A definite relationship was noted between the time of performance of external cephalic version and the incidence of reversion to breech position, as shown in Table 1.

TABLE 1

TIME OF PERFORMANCE OF EXTERNAL CEPHALIC VERSION IN RELATIONSHIP TO SUBSEQUENT REVERSION

Week of pregnancy	*Number of cases*	*Number of reversions*	*Per cent of reversions*
Before 32	15	7	47
32 to 34	32	10	31
34 to 38	30	6	20
After 38	6	0	0

TABLE 2

APPROXIMATE TIME OF OCCURRENCE OF SPONTANEOUS CEPHALIC VERSION

Week of pregnancy	*Number of cases*	*Number of reversions*
Before 31	18	0
31 to 32	11	2
32 to 33	3	0
After 33	2	0
Total	34	2

When reversion occurred it usually occurred early, 20 of the 23 reversions being noted on the first visit after external version had been done.

Spontaneous Cephalic Version

Spontaneous cephalic version was noted in the last trimester of pregnancy in 34 patients, in two of whom it occurred after an unsuccessful initial attempt to turn the infant externally. Spontaneous version happened between office visits, so the time of its occurrence can be approximated only. In Table 2 it is assumed that the change of position took place midway between the time it was first noted and the preceding visit.

Some instances of early spontaneous version were not recorded, as previously explained. The incidence rates are further distorted by the prac-

tice of external cephalic version, since many infants versed prophylactically would have turned spontaneously if left alone. Table 2 therefore does not indicate the true frequency of this movement, but it is noteworthy that in this group of early spontaneous versions there were only two instances of reversion to breech position. Both were later turned externally and then remained in vertex position.

Spontaneous Podalic Version

Forty-three instances of spontaneous podalic version were noted during the last trimester. Most of these occurred as reversions in the group of patients in whom external cephalic version had been done previously. In this group, reversion to breech position occurred one or more times in 17 patients, for a total of 23 spontaneous podalic versions. Since there were

TABLE 3

APPROXIMATE TIME OF OCCURRENCE OF SPONTANEOUS PODALIC VERSION

Week of pregnancy	*Number of cases*
Before 32	8
32 to 34	14
34 to 36	9
36 to 38	10
After 38	2

60 patients, and 83 external versions in this group, the incidence of spontaneous podalic version following external cephalic version was 25 to 30 per cent.

This is in striking contrast with the low incidence of spontaneous podalic version in other circumstances. For all other cases of the series, the incidence was 4 to 5 per cent, and among the 34 patients who had spontaneous cephalic versions, only 2 reverted, an incidence of 6 per cent.

The approximate time of occurrence of spontaneous podalic version is given in Table 3.

It would appear from this that cephalic presentation is relatively stable after 38 weeks of pregnancy.

Breech Deliveries

There were two instances of breech delivery in the series, an incidence of 0.4 per cent. One terminated in delivery of a premature infant, the other of a hydrocephalic monster. The histories of these follow.

S.M., a 30-year-old grava 2, para 1, had a spontaneous rupture of the membranes at 28 weeks of pregnancy, at which time breech presentation was noted. Version was not attempted then or subsequently. Treatment consisted of bed rest and antibiotics. A slow leakage of fluid continued for 2 weeks. Labor then began, and the patient was delivered spontaneously of a 2250-gram boy, who survived.

G.M., a 27-year-old primigravida, was noted to have a breech presentation at 34 weeks of pregnancy. The uterus was unusually tense, the breech deeply engaged, and repeated attempts to turn the baby were unsuccessful. During the course of labor it soon became obvious that the fetus was grossly hydrocephalic. This deformity had not been diagnosed previously, though in retrospect it should have been suspected. The infant died intrapartum, and was then easily extracted following decompression of the skull with Smellie scissors.

Cesarian Sections

Two of the patients in whom external cephalic version had been done were later delivered by Cesarian section, one for a coincidental ovarian cyst occupying the cul-de-sac and the other for placenta previa. In no case was Cesarian section done because of breech presentation, and all infants delivered abdominally, for whatever indication, were in vertex position at the time of operation.

Fetal and Maternal Complications of External Cephalic Version

The 60 infants delivered after external cephalic version were all liveborn. One died on the second day of life of multiple congenital anomalies incompatible with survival.

There were no instances in this series of maternal bleeding, prolapse of the cord, rupture of the membranes, induction of premature labor, or other harmful effects that could be attributed to the operation. Version was performed twice without apparent injury upon a patient subsequently found to have placenta previa, and she was delivered of a live baby by Cesarian section 4½ weeks after the second version.

Discussion

Breech presentation is found in a fifth to a quarter of all gravid women at some time during the last trimester. As pregnancy advances, many of the breeches turn spontaneously to vertex, while the reverse movement occurs much less frequently. The incidence of breech presentation therefore declines progressively as term approaches, usually reaching a level of about 4 per cent at the time of delivery.

The motive power for spontaneous version is undoubtedly furnished by active muscular movements of the fetus itself. Patients are often aware of the occurrence of spontaneous version and remark upon the vigorous fetal activity which accompanies it. During the process of external version, the baby will sometimes be stimulated to strenuous movement and literally kick itself around, completing the version by its own muscular effort. Moreover, the author has never observed spontaneous version of a fetus dead *in utero*. Indeed, repeated X-rays in such cases usually show an extraordinary fixity of attitude and position.

Spontaneous change of fetal polarity was observed with dissimilar frequency in different circumstances, indicating that it is not a purely

random movement. This effect may be attributable in some measure to the relationship of the fetal legs to the presenting part. In full breech presentation, the juxtaposition of the feet and buttocks at the pelvic inlet results in a particularly unstable situation, since kicking movements tend to dislodge the breech, and a version thus initiated may progress to completion. Less displacement of the presenting part may be anticipated from kicking movements in cases of frank breech or of vertex presentation, and indeed the frequency of spontaneous version declines in this very sequence. Spontaneous podalic version was observed far more frequently after external cephalic version than after cephalic presentation assumed spontaneously by the fetus. This indicates that accommodation of the fetus to the uterus has an important rôle in determination of fetal position. In most instances, the infant seems to "fit" best in vertex position, perhaps because of the shape of the uterine cavity, the fetal posture, or other unknown influences. Such babies assume the vertex position initially or reach it by a single, early movement of spontaneous cephalic version, rarely reverting thereafter, as Vartan[18] observed. In a minority of cases, on the other hand, the infant seems to "fit" better in breech position. Such babies tend not to undergo spontaneous cephalic version, and if turned externally, they often revert, quickly and sometimes repeatedly, apparently preferring the breech position.

It is not proposed that breech position is favorable for these infants, or that the obstetrician should resign himself to the vagaries of fetal preferences. These are the very infants which must be versed if the frequency of breech delivery is to be reduced, since the others would probably turn of themselves. If there were an accurate guide for prognosticating spontaneous versions, the obstetrician could limit his efforts to the small group of cases that would otherwise deliver as breeches. Unfortunately, no such guide is known, so some babies are turned unnecessarily in the endeavor to verse those that would not have turned spontaneously.

The optimum time for the first attempt at external cephalic version is usually about 32 to 34 weeks of pregnancy. If it is done earlier, a higher rate of reversion may be expected, and the benefit of early spontaneous versions will be lost. Done later, the operation is more difficult, and an increasing number of prematures will deliver as breeches.

External version was considered contraindicated in a patient with ruptured membranes. Bleeding in the last trimester, an unusually tender or irritable uterus, or a history of premature labor may also be regarded as contraindications, though not absolute. Version is not done in cases of multiple pregnancy and may be omitted in patients to be delivered by Cesarian section. Frank breech presentation is not considered a contraindication to external version. The maneuver is more difficult to perform, however, if the fetal legs are extended, and is also more difficult if the breech is deeply engaged, the uterus tense, or the amniotic fluid insufficient or excessive in amount.

Summary and Conclusions

1. External cephalic version was done routinely and repeatedly when necessary in a series of 500 consecutive private patients each delivered of a single fetus weighing 1500 grams or more at birth.

2. The number of breech deliveries was reduced to two, an incidence of 0.4 per cent. One of these infants was premature, the other hydrocephalic. All normal term babies were successfully turned.

3. There was no maternal or fetal mortality or morbidity in this series attributable to external version.

4. External cephalic version is a safe and effective means of reducing the frequency of breech deliveries and deserves wider application.

5. The importance of some of the factors influencing fetal position *in utero* may be judged from observations of spontaneous versions, both cephalic and podalic. These are discussed.

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UNRECOGNIZED INTRAUTERINE ANOXIA AS A CAUSE OF FETAL DEATH DURING LABOR

HOYT C. TAYLOR*

Although impressive progress has been made in the reduction of maternal mortality there has long been recognized a lack of concomitant reduction in fetal deaths. Only recently have efforts been specifically applied to the problem of reducing fetal mortality, although general progress in medicine has lowered the neonatal death rate from 44 per 1000 live births to 24 per 1000 during the last twenty-five years.

Although toxemia, prolonged labor, traumatic delivery, maternal anemia, excessive analgesia and anesthesia, and placental accidents have long been recognized as causes of intrauterine anoxia and fetal death, relatively little attention has been directed toward labor itself.

The production of anoxia by over-forceful labor may well account for some of the so-called "unexplained fetal deaths in utero." Traut,[6] who has long been a student of placental physiology, states that oxygen saturation of fetal blood constantly diminishes toward term and in the last trimester falls from 50 to 28 per cent saturation. Such pathological changes as hemovillus degeneration may add to fetal hypoxemia and this process may be hastened by toxic states or poor nutrition, especially vitamin deficiencies.

Beck[1] has shown that oxygen normally supplied to the fetus is relatively inadequate. The high red blood cell count of the fetus is evidence of a compensatory effort to combat this hypoxemia. The adequacy of the placenta varies with its placement in the uterus, its maturity, and the demands put upon it in labor. Knaus[3] has noted in binovular twins of distinctly varying weights and size that in all instances these variations were proportionate to the placental weights. If a fetus is of excessive size, it follows that its demands upon the relatively inadequate oxygen supply at term are greater, and if gestation is prolonged, the placenta, as pointed out by McKiddie,[5] becomes progressively less efficient. The stillbirth rate is often thus increased.

With this physiological background it is not surprising that when there are exceedingly strong, hard, and frequent labor contractions, intrauterine anoxia may occur, leading to "unexplained deaths." Precipitate, or strong and forceful, rapid, driving labor added to the known deficiencies inherent in placental physiology can bring about fetal death in utero. Moreover, this may happen repeatedly in the same patient. Even if death does not occur,

* Instructor in Obstetrics and Gynecology (Resident, New Haven Hospital), 1942-43.

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there may be irreversible neurological damage which causes death in the early neonatal period. Prolonged and frequent labor pains may prevent the blood in the placental sinuses from being renewed adequately, although the maternal blood contains a sufficient supply of oxygen. Even if he escapes severe anoxia, the infant may later suffer from serious behavioral difficulties, subnormal intelligence, and possibly epilepsy. Fender[2] found in two-thirds of a series of patients with idiopathic epilepsy that there was definite evidence of asphyxia, whereas in a control series only 10 per cent of the patients had any history of this. However, some workers have been unable to confirm Fender's results.

Case Histories

Case I. A 35-year-old female had the following obstetrical history. Her first pregnancy ended in an abortion at three months. The second pregnancy was uneventful, with the onset of labor two weeks before term. This was strong from the onset with pains every two minutes. The first stage lasted nine hours, the second stage, 26 minutes. The patient had little rest between the uterine contractions in spite of nembutal 0.4 gm. The baby was stillborn. A necropsy was not obtained.

The third pregnancy terminated in premature labor at 37 weeks. Labor again was characterized by strong pains, the first stage lasting 17 hours. A 3-lb.-14½-oz. female was delivered spontaneously. Sedation consisted of nembutal 0.3 gm. and demerol 100 mgm. The baby was in fairly good condition at birth, doing well up to the fourth day when a sudden cyanotic episode occurred requiring oxygen therapy which gradually improved the infant's condition. Subsequently, this child developed a severe form of epilepsy.

The fourth pregnancy was complicated by a mild hypertensive toxemia. Labor of a strong character with contractions recurring every two minutes began at term. The first stage lasted five and a half hours, the second stage, 20 minutes. A male was delivered spontaneously, weighing 6 lbs. 2¾ oz., and was apparently normal except for a slightly grunting type of respiration. The baby died suddenly on the second postpartum day. A post-mortem examination was refused. The fifth pregnancy was terminated by elective cesarean section prior to the onset of labor, two weeks before term. A 5-lb.-11-oz. male did well from birth and has been normal to date; he is now five years of age.

Case II. A 33-year-old primigravida after an uneventful pregnancy went into spontaneous labor of an exceedingly strong character with rapidly recurring pains. The pelvis was found adequate by x-ray pelvimetry. At one to two cm. dilatation, meconium-stained amniotic fluid was noted. Contractions were recurring at one-and-a-half to two-minute intervals and lasting one full minute or longer. Sedation consisted of demerol 100 mgm. and nembutal 0.2 gm., and labor was slowed with open drop ether. The first stage lasted seven hours, the second stage was terminated in 30 minutes by low forceps under spinal anesthesia. The fetal pulse disappeared suddenly just prior to delivery. A dead female weighing 7 lbs. 10 oz. showed at necropsy findings usually associated with anoxia—scattered small petechial hemorrhages on the surface of the lungs, heart, and thymus. There were no other complications to account for this infant's intrauterine anoxia.

Case III. A 30-year-old primigravida after a five-year period of sterility responded to treatment and became pregnant. Her antenatal course was complicated by strong Braxton Hicks contractions of which the patient was very conscious. X-ray pelvimetry revealed an adequate pelvis. At term, labor occurred spontaneously and after a 12-hour

first stage, characterized by strong, forceful pains recurring every two to three minutes, meconium was noted. Low forceps under spinal anesthesia terminated the labor shortly after the onset of the second stage. A male weighing 7 lbs. 6 oz. was delivered and survived. There were no cord complications or other known causes of fetal distress other than the character of the labor. The child has developed normally. A second pregnancy resulted in an abortion at six weeks (blighted ovum).

A third pregnancy was complicated by a threatened abortion at three and a half months. Labor began ten days beyond term and contractions were strong and hard, recurring every two to three minutes. The first stage lasted only one hour and 55 minutes, the second stage was terminated after 27 minutes by low forceps under spinal anesthesia. A female weighing 7 lbs. 4½ oz., covered with meconium, was pale and cyanotic but improved with oxygen therapy and appeared to be progressing normally. However, the infant underwent a sudden cyanotic spell at the end of 24 hours and in spite of all treatment died. Necropsy was not obtained.

Discussion

All three cases had in common a forceful type of driving labor typified by strong pains lasting at least a minute and recurring at about two-minute intervals. It is apparent that this type of labor may well be inherent in an individual and may recur in successive pregnancies. In most instances it is a fetal death, in the absence of other factors known to cause fetal anoxia, which first draws attention to this specific problem. One of the cases presented was conscious of strong Braxton Hicks contractions which may have been of significance.

In all three instances the appearance of meconium-stained amniotic fluid served as an indication of intrauterine fetal anoxia. Unfortunately, once fetal distress has been noted, the damage may already have occurred, although subsequent treatment may enhance the possibility of recovery. As has been noted in similar situations, the infant may survive the anoxia only to succumb to the after effects in the early neonatal period. If it does survive, then the possibilities of subnormal intelligence, spastic paraplegia, serious behavior difficulties, and epilepsy may be present.

Theoretically, the same explanation may be offered for the most outstanding example of intrauterine fetal loss with which we have to contend, namely, that which occurs in the diabetic and prediabetic mother. Abnormal metabolic processes may well account for the delay in the onset of labor, while in the diabetic the high glucose content of the maternal blood contributes to the excessive size of the fetus.[4] Thus we have an oversized fetus going beyond term and requiring more oxygen than can be supplied it by a placenta known to become more inefficient as it becomes postmature. A contributing factor is probably the arteriosclerosis present in these patients, sometimes in the severe diabetics to such a degree as to render the placenta small and unable to support the fetus beyond the sixth or seventh month of gestation. In the event that it is maintained in utero, there would therefore be a much reduced margin of safety in oxygenation so that the fetus might well succumb to anoxia brought about by the stress of even normal labor.

At the present time the diagnosis of labor of such severity as to be incompatible with fetal safety can only be made in most instances after the occurrence of the first fatality. And then only when all other known factors have been ruled out. In time an etiological factor, perhaps of endocrine origin, may be discovered which will enable the obstetrician to foresee this situation; at present we can only profit by past experience.

Since this type of labor appears to be inherent in an individual and to recur with subsequent pregnancies, one is faced with the problem of how to prevent a second fatality. Experience has shown that these patients have a short, rapid labor by virtue of their too efficient uterine contractions, hence sedation strong enough to slow them down must of necessity be given in large amounts and early. This is not very practical for although it may be possible to accomplish the desired end, it may well bring about such depression in the fetus that the ultimate result is the same. Also in many such instances sedation has little effect on this type of labor and only seems further to embarrass the fetus and make more difficult the problem of resuscitation. The trend at present is away from strong sedation as an answer to the problem.

The only logical solution appears to be the elimination of labor by resorting to elective cesarean section in the occasional carefully selected case which fulfills the above criteria.

Conclusions

Three cases have been presented having in common fetal death from intrauterine anoxia as the result of an extraordinarily strong, driving type of labor.

Elective cesarean section in carefully selected cases seems at present to be the method of choice in preventing a second fetal death.

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THE THEORY AND PRACTICE OF NATURAL CHILDBIRTH

FREDERICK W. GOODRICH, JR.*

Now that six years have elapsed since the study of natural childbirth was begun at Yale it seems appropriate to review the status of this philosophy in the perspective of time and experience. Not only is this appropriate, but it is necessary in view of the continuing enthusiasm of its proponents and the continuing attacks of its opponents. Is it a universally objectionable, backward step in the forward progress of obstetrics or a universally applicable and beneficial technique? Is it a complex ritual suitable only for the obsessive, dependent woman or are its routines simple enough and sufficiently valuable to merit incorporation in all prenatal programs? So much has been written and said supporting both these points of view and the controversy has spilled over into the lay press so frequently that it becomes important that both obstetricians and patients have a clear understanding of its basic principles and applications. Such an understanding is doubly important if, as even its opponents admit, natural childbirth has certain advantages, lest these advantages be discredited in the heat of controversy.

To me, natural childbirth is basically a system of intellectual, emotional, and physical preparation for childbirth to the end that mothers may enjoy a healthier and happier pregnancy and delivery. That these aims are difficult, if not impossible, to evaluate objectively is admitted, but does this mean that attempts to reach these goals are useless or that we must deny clinical judgment because it cannot be proven (or disproven) in the laboratory? The experience of those who have made a genuine attempt to apply the techniques of natural childbirth seems to prove otherwise since their numbers increase every year. More and more hospitals are adding prenatal classes to their maternity care programs, more and more frequently the subject is appearing on medical programs, and recently the New York City Department of Health recommended that all hospitals with prenatal clinics organize parents' classes.

A recent article entitled "Is Natural Childbirth Natural," by Mandy *et al.*[8] epitomizes the objections of the opponents of this philosophy. Recognizing the fact that "Read's basic concept, that fear of childbirth increases the patient's tension state and thereby produces pain in labor, is sufficiently broad for general acceptance," they go on to attack the means of combatting

* Graduate student and Assistant in Obstetrics and Gynecology (Assistant Resident, Associate Resident, and Senior Associate Resident, Grace-New Haven Community Hospital University Service), 1946-49.

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and overcoming this fear. These means are described as "diet, education, exercise, and relaxation." Let us see how this theme is developed.

Since no American investigator has ever stated that diet plays a rôle in influencing anxiety in labor this can be dismissed in as many words. The authors then state that "education is of value only in the desensitization of some of the common taboos surrounding pregnancy and the childbirth process. Those who have attempted too intensive a program of education in anatomy and physiology are aware that such a procedure can arouse as much anxiety as it allays." Since some anxiety is produced by the common taboos one would think that any efforts to desensitize patients would be worthwhile; furthermore, if attempts at education arouse more anxiety than they allay, the obvious conclusion would be that the educator was at fault rather than the premise. Routine prenatal care consists in education. When we prescribe a diet for a patient we are educating her and occasionally we may arouse anxiety when we explain the reason for a certain diet, unless we take pains to present it in such a way as to preclude arousing anxiety. Herein lies the necessity for skillful education. Simply because some patients react with anxiety to unskillful education we do not conclude that such education is universally undesirable. That there is an increasing interest on the part of our patients in learning more about anatomy and physiology is attested by the increasing numbers of articles on these subjects in the popular press. I believe that the day is past when we could expect blind obedience from our patients simply because we direct them to follow a certain routine. I have heard it said that it is not up to us to educate our patients because it should have been done in the schools!

The "unbridled" publicity which is so heartily condemned is but another example of the avidity of the lay public for information about themselves. In most instances articles emanating from medical sources have been misinterpreted and misquoted no matter how carefully screened beforehand. If we as physicians fulfill our rôle as educators, then perhaps there will be less misunderstanding on the part of our patients and the exaggerations of the lay press will be less effective.

The exercises are described by the authors of this article as "not only a nuisance . . . but they are impossible to evaluate objectively. . . ." Many of the time-honored and proven techniques of prenatal care such as the taking of vitamin-mineral supplements and the adherence to prescribed diets are undoubtedly a nuisance but are they discarded on that basis? Many patients obviously do not follow their obstetrician's advice for various reasons, but do we discontinue the giving of advice because of the lack of co-operation of the few? Can we evaluate objectively all of the routines we now utilize in modern prenatal care? The exercises commonly used in natural childbirth programs have been adequately described elsewhere.[1, 3, 4] As one who has seen the pelvic rocking exercise relieve the postural backache of preg-

nancy in over ninety per cent of patients, I am not inclined to discard its use simply because objective valuation is lacking.

The authors continue by attacking the relaxation techniques which "form the cornerstone of every natural childbirth program." This takes the familiar form that these techniques are to be condemned because they induce hypnosis. They quote Pascal to the effect that "simple relaxation seems to be sufficiently far along on the hypnotic continuum to facilitate recall and to increase the suggestibility of the patient." With this I am in complete agreement. However, does this condemn the use of relaxation techniques? If, by having the patient practice relaxation during pregnancy and by reinforcing this practice very simply in labor we can significantly decrease the pain she may feel, then I would conclude that this is a highly desirable thing to do. Although deep hypnosis will accomplish the same thing, its use has several disadvantages. First, the stigma attached to its use, as described by the authors, is a very real factor. Second, the time required with each patient prenatally is much greater than most doctors have available; and third, less than fifty per cent of the population at large by the most generous estimate are capable of being deeply hypnotized. There are doctors who have abandoned hypnosis in favor of natural childbirth techniques because of the relative advantages of the latter over the former.[6]

One of the simplest and most effective of the relaxation techniques employed is diaphragmatic or "abdominal" breathing. Most effective if practised prenatally, it is still of significant value even though it is first utilized in labor. It is my belief that well over ninety per cent of patients experience distinct relief in the first stage from its use. In the hospital where I practise, the obstetrical nurses have found it so effective that they usually instruct the patients of other doctors in its use during labor. I am convinced that this will decrease significantly the amount of drugs necessary. Simply labelling this an hypnotic technique does not destroy its value but it may well serve to stigmatize natural childbirth in the minds of women and doctors who are unfamiliar with either.

The article goes on to state that anxiety in pregnancy is not the result of the single emotion of fear of childbirth. While this is undoubtedly true it can still be said that fear of childbirth is all too common in pregnancy and has a significant effect on labor and delivery. Recognizing this, it seems logical to do what we can to overcome this fear. We can hardly expect, as obstetricians, to correct basic personality defects by our treatment in pregnancy, but there is no reason why we should not do what we can with available techniques to mitigate such anxiety as is within our reach. In fact, I believe that we are being derelict in our duty to the patient when we simply treat her pregnancy and do not treat her as a patient who is pregnant.

In describing the natural childbirth program in Dr. Thoms' Clinic at New Haven the authors admit that a genuine attempt has been made to substitute "negative conditioning in pregnancy with a positive one." By implication they admit that possibly this is worthwhile but even so, "the program must be supplemented . . . by substantial amounts of analgesia and anesthesia. The total amount of drugs administered to any patient is determined by the individual's needs and is limited only by the restriction that *she must be kept conscious during her labor.*" [Italics mine.] This is an expression of the common misconception that for natural childbirth to be "successful" the patient must be conscious at delivery. This misconception arises, no doubt, not only from the writings of Grantly Dick Read, but also from the publications from Dr. Thoms' Clinic in which consciousness at delivery was simply used to illustrate the minimal amounts of analgesia and anesthesia which most patients require. It has been my experience both at New Haven and in private practice that with natural childbirth training from fifty to eighty-five per cent of patients do not need to be deeply anesthetized although this is not primarily an aim of the program. I customarily ask patients on the delivery table whether or not they want to go to sleep. If the delivery is obstetrically normal, the choice of how much anesthesia to be given is thus up to the patient. Frequently, when rotations are necessary or arrest has occurred I ask the patient to take an anesthetic although she herself feels that she does not require it.

The concept of success or failure in natural childbirth is an artificial one and, unless patients are reassured, can give rise unnecessarily to guilt feelings in those patients who for one reason or another cannot be awake at delivery. There should be no prestige attached to conscious delivery nor lack of it when mothers are not awake.

In their first conclusion the authors of the article in question reiterate another common misconception—that arising from the designation given to this philosophy. They say: "Natural childbirth, as it is being interpreted today, bears little resemblance to the procedure under which name it masquerades." While I agree that the term "natural childbirth" is unfortunate in its connotations, usage seems to indicate that it will continue to be known as such. Certainly attempts to substitute terms such as physiological childbirth or other euphemisms have been unsuccessful. However, it seems unnecessary and misleading to condemn the entire system because of a semantic disagreement over designation. Calling the relaxation techniques hypnosis is another example of semantic quibbling.

Traditionally, prenatal care has been one-sided in its emphasis on the physical. Natural childbirth is a philosophy of *total* preparation in which the mother is intellectually and emotionally prepared as well. If such is accomplished, it has been the general observation that prepared mothers are more comfortable during pregnancy, labor, and delivery, and enjoy the

experience to a greater degree than those who are not exposed to such preparation. As a consequence of this training more mothers need less help from drugs and anesthetics than has heretofore been the case. It would seem to follow that if any great experience such as childbirth were made more pleasant and satisfying, certain psychological advantages would ensue.

The point should be made that the training involved does not have to be "complex" or "ritualistic." In clinic practice, group meetings are the obvious way to teach. In private practice there are probably as many methods of training as there are doctors practising this approach. Many of us have found that having the patients read books and pamphlets and reinforcing the reading with personal instruction at office visits are simple enough to be both practical and to meet the needs of most. In some communities there are classes taught by nurses or sponsored by maternity health organizations to which private patients can be sent. In any community the hospital can and should be the place where parent education is centered. Classes incorporating the use of visual aids such as movies conducted by the obstetrical house staff or the obstetrical nursing department or both should be simple to organize, of obvious value to the patient, and a source of good public relations for the hospital. They serve the further function of bringing the nursing department into the program. Without nursing participation any natural childbirth program is far from complete since the reinforcement of the training during labor is essential. This reinforcement comes in large part from the obstetrical nurses. It should be emphasized that individualization is not only desirable but absolutely necessary. This can be done in the doctor's office in conjunction with the education by group classes.

Thoms and Wyatt[5] have stated that no doctor who has ever given this approach a trial has been willing to abandon it. As far as I am aware this statement is still true and even the authors of "Is Natural Childbirth Natural?" do not indicate that they intend to discontinue the program which they are apparently conducting. We must assume that this approach has some value not found in the usual prenatal care routines or that a large number of patients and doctors are misguided.

Conclusions

1. The fact that methods are lacking to evaluate objectively values such as satisfaction, contentment, happiness, and their opposites should not be allowed to discredit sincere attempts to improve the lot of the patient in childbirth.

2. Semantic confusion is responsible for beclouding the basic issues in the natural childbirth program.

3. Undue concern over questions such as whether or not pain is "necessary" in normal labor or whether or not relaxation techniques are hypnosis can discourage the more general adoption of techniques which are admittedly useful.

4. The essence of natural childbirth is its promulgation of a philosophy of total prenatal care. As such it is applicable to all patients.

5. This program can materially reduce the necessity for heavy and undesirable medication in labor.

6. In the clinical judgment of those who have attempted it education of mothers for childbirth has been found to be of sufficient value to warrant general adoption.

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EVENTS

BIOCHEMISTRY SEMINAR

February 17, 1953

ION EXCHANGE CHROMATOGRAPHY APPLIED TO AMINO ACID CONTENT IN URINE. By William Stein, Rockefeller Institute for Medical Research, New York, New York.

By providing a degree of quantitative accuracy not attainable with earlier methods, the application of ion exchange chromatography in the determination of the amino acid content of urine is proving valuable not only as a clinical diagnostic tool, but in the confirmation and extension of previous observations as well.

Samples are taken from 4-hour specimens, concentrated to 4 cc., brought to a pH of 2.5, and introduced into a Dowex 50, 1x100 cm. resinous ion exchange column. Using an automatic fraction collector, the eluant from the column is collected as a large series of fractions of equal volume. The amino acid concentration of each fraction as determined by the ninhydrin reaction is plotted against the eluant volume, with the resulting curve consisting of a series of peaks. Each peak represents an individual amino acid.

Positive identification of an amino acid by this method demands satisfaction of the following criteria: (a) complete superimposability of the peak of the unknown with that obtained by adding a known amino acid to the urine sample; (b) stability of the peak to acid hydrolysis of the urine sample, i.e., it must remain in the same relative position and undiminished (the area under the curve is a quantitative measure of the concentration of the amino acid), though an increase is possible, since hydrolysis may cause the appearance of free amino acids not originally present as such; (c) confirmation by paper partition chromatography.

The results obtained by this method suggest that the quantitative pattern of urinary amino acids is specific for the individual. Moreover, of the twenty-odd amino acids known to occur normally in human urine which are excreted at a rate of approximately 1 gm. every 24 hours, four of these—taurine, glycine, histidine, and methyl histidine—comprise 70% of the total excretion.

Of particular clinical importance is the finding that in cystinuria, a condition often associated with renal calculi, dietary cystine seems to be normally accommodated; the high cystine content of the urine is thought to be due to (a) an abnormal metabolism of cysteine, methionine, ornithine, and taurine leading to production of cystine, and (b) a loss of the ability of the renal tubules to resorb cystine.

Hepatolenticular degeneration (Wilson's Disease) is usually fatal and exhibits a syndrome characterized by limb tremor and rigidity, pigmentation of the cornea, and cirrhosis of the liver. As these symptoms are widely inclusive and far from specific, chromatographic urinary amino acid analyses may prove helpful in diagnosis. The urine of Wilson's Disease victims is characterized by the following irregularities in amino acid concentration: first, a total amino acid excretion seven to eight times above normal; second, exceedingly large amounts of threonine, glycine, and serine; and third, the presence of citrulline, not encountered in normal urine.

J. C. C.

PLANT SCIENCE SEMINAR

February 26, 1953

IN DEFENSE OF GENE MUTATIONS. By Adrian M. Srb, Department of Plant Breeding, Cornell University, Ithaca, New York.

Geneticists work with mutations that are relatively striking, most of which are detrimental to the organism. However, many micro-mutations occur which are detected only under exceptional circumstances; they may benefit the organism in certain specific situations.

The amino acid canavanine inhibits growth of the mold *Neurospora crassa* by competing with the structurally related amino acid arginine. Corn embryos are similarly inhibited by canavanine; the inhibition can be negated by supplying the organism with arginine. Work with various strains of corn shows that each differs in its susceptibility to canavanine. Crosses between some of these strains have produced hybrids that are superior to either parent in their resistance to canavanine inhibition; however, these changes in resistance could not be associated with specific genes.

A considerable number of canavanine-resistant strains were obtained in *Neurospora* and genetic data showed that the resistance resulted from a mutation at a specific locus. This mutation, detectable only in the presence of canavanine, superficially differed in no way from the original parent strain. Further breeding experiments in which a canavanine resistant strain was mated with a strain requiring arginine indicated that resistance to canavanine was associated with a more efficient utilization, i.e., a lower requirement for arginine.

Another example of a non-deleterious gene change was provided by an ultraviolet-induced mutant of a strain requiring arginine. The original arginine requirer was inhibited by comparatively low concentrations of the amino acid lysine. The mutant, still arginine dependent, was resistant to much higher concentrations of lysine. Again, this type of mutation would not be detected under ordinary circumstances.

In *Neurospora,* mutations of no immediate selective advantage to the organism might be preserved by heterokaryosis. Under conditions favoring the new mutant gene, nuclei carrying the mutation would then be selected.

HOWARD V. RICKENBERG

PHARMACOLOGY SEMINAR

March 26, 1953

METABOLIC REQUIREMENTS OF SYMPATHETIC NEURONS WITH SPECIAL REFERENCE TO ANESTHETICS. By Martin G. Larrabee, Department of Biophysics, The Johns Hopkins University, Baltimore, Maryland.

Determinations of the metabolic requirements of the superior cervical sympathetic ganglia of rabbits and rats, either excised or *in situ*, have shed light on the possible level of action of anesthetics. The ganglia, bathed in a solution containing (along with other essential physiological constituents) 5% CO_2, 95% O_2, and mEq. of glucose, are subjected to varying degrees of stimulation or anesthesia, and the bathing solution then analyzed for changes in concentration of oxygen and glucose.

Work with unanesthetized ganglia reveals (a) that of the various metabolic requirements, O_2 is the most important. As the rate of stimulation is increased, the O_2 consumption increases to a maximum consumption

of 40% above normal at 10 per second. Under comparable conditions somatic nerves show an increase of only 6%. This is thought to be due to an inherent difference in the metabolic activity of sympathetic as compared to somatic fibers, possibly as a result of their greater size and not, as was formerly believed, the result of the fact that ganglia contain cell bodies and dendrites as well as nerve fibers. (b) Glucose uptake is also essential to the integrity of the ganglion, the lack of which results in partial or complete loss of conductivity. Furthermore, this loss is more marked in post- than in pre-ganglionic fibers, indicating an influence in the ganglion. There is an irreversible effect of the lack of glucose; unmyelinated fibers are significantly more susceptible than myelinated. This is due to a "reserve" material, possibly glycogen, in the myelin sheath.

Anesthetized preparations indicate that some of the functions of sympathetic ganglia decrease with increasing quantities of an anesthetic and that for comparable effects to be induced in somatic fibers, much higher concentrations must be used. It is therefore significant that those anesthetics known to have the strongest action show great selectivity for synaptic blocking. Since very few anesthetics have any demonstrable effect on O_2 uptake, it is possible that anesthesia may be the result of influence upon other phases of ganglion metabolism. It has recently been observed that certain anesthetics, such as pentobarbital, result in increased glucose utilization.

J. C. C.

HISTORY OF MEDICINE LECTURE

April 16, 1953

THE CONCEPT OF INFECTION BEFORE PASTEUR. By Owsei Temkin, Institute of the History of Medicine, The Johns Hopkins University, Baltimore, Maryland.

Etymologically the word "infection" and related terms stand for "strain" or "impurity." As such they have been used for many centuries with but loose connection with infectious disease. It was only in the Middle Ages that the medical concept was more clearly elaborated.

Infection (by our present definition) and preventive measures against it are perhaps first discussed in the Old Testament in connection with leprosy. The public health measures suggested are sound and even extreme, but the reasoning behind them seems to be chiefly religious. Hippocrates' school dropped the concept of the heaven-sent disease as the cause of contagion in favor of the defilement of air by some miasma. The rôle of the stars in producing plagues has also been considered predominant from antiquity through the Renaissance. However, parallel with these theories there also was developing a popular classification of disease into "clean" and "unclean" and, as time passed, many of our present infectious diseases found their way into the unclean category. Nevertheless, progress in public health proceeded on the theory of airborne miasma; to remove bad odor, supposedly containing the miasma, fumigation and the use of perfume came into wide use.

In the sixteenth century Fracastoro implicated small particulate matter in the transfer of contagion via fomites or by direct contact, but there was little progress from this point until the mid-nineteenth century.

The greater emphasis on cleanliness in the last century was a condition for the willingness to accept the results of Schwann and Henle and later those of Pasteur, Koch, and Lister. Through their work the old idea of

pollution became replaced by the scientific concept of infection. This concept still implies our readiness to prevent and combat what is considered an evil and not a mere scientific phenomenon.

N. A. H.

HISTORY OF MEDICINE LECTURE

April 17, 1953

THE PARADOX OF PARACELSUS. By Owsei Temkin, Institute of the History of Medicine, The Johns Hopkins University, Baltimore, Maryland.

Philippus Aureolus Theophrastus Bombastus Paracelsus ab Hohenheim is a compelling figure in the history of medicine. As a medical reformer he overthrew the errors of his predecessors to substitute those of his own. As an observant scientist he deemed a philosophy of medicine to be of prime importance. And although a physician, he has long been remembered as a "magician" as well. In short, he is a paradox.

Born in 1493, he received his early medical education from his physician-father and a religious order. Subsequently he depended solely on his own observations and speculations, eschewing the classical works of Galen and Aristotle.

An example of his early thought is found in the *Volumin Paramirum* in which he set forth the five factors that determine health and disease: (i) The astrological factor. Man, he said, is under the influence of the stars. Since some diseases are peculiar to certain seasons, man's affliction with them is governed by the changing calendar. (ii) The poisonous factor. Man is influenced by poison, it being anything and everything he consumes. If, in digestion, the right food substances are not separated out and retained by the body, sickness results. (iii) The natural factor. Certain diseases are associated with each of the ages of man. (iv) The spiritual factor. Every man is related to other men in a spiritual way. Mental disease is a result of the disruption of these relationships. (v) The divine factor. All diseases are sent from God but He also sent physicians to cure these diseases, so no case should be given up as hopeless and the doctor must continue to hunt for the remedy. His emphasis on the idea that research is an integral part of the physician's task certainly anticipated its importance in modern medical thinking.

In 1527 Paracelsus was invited by the city council of Basel to become town physician and a professor of the university. But his stay in Basel was short-lived; he and the council could not see eye-to-eye.

His activity from 1529-1538 was varied. He wrote on the use of quaicum in the treatment of syphilis and he composed his great treatise on surgery (1530) in which he advocated the abolition of ointments in treating wounds, advising instead the use of a sympathetic powder. In 1538 he published his last works, one of which, *Labyrinthus Medicorum Errantium*, is especially noteworthy. The erring physicians were, of course, all those except Paracelsus. He criticized them for making speculations without observations and for learning from textbooks rather than from observation. But he also tried to explain many diseases on the assumption that man was a microcosm reflecting the macrocosm. Thus epilepsy was "explained" as a thunderstorm of the body.

In 1541 Paracelsus, old, broken, and lonely, died in Salzburg, a "magician," a "miracle healer," but always a searcher.

G. A. F.

MICROBIOLOGY SEMINAR

April 17, 1953

THE DEVELOPMENT OF RESISTANCE TO CYCLOHEXIMIDE IN *Allomyces arbuscula*. By Alma Whiffen, New York Botanical Garden, Bronx Park, New York, New York.

The activity of cycloheximide (actadione) in suppressing the growth of the fungus *Allomyces arbuscula* in synthetic medium equals its effectiveness in complex media containing yeast extracts. In concentrations as small as 0.25 μ gm./ml. of nutrient agar, the per cent survival of spores is 90, which decreases progressively to zero at concentrations greater than 20 μ gm. The effect of the antibiotic is not only on the growth rate but on the production of new branches from old hyphae and on the length of time required for spores to germinate, as well as the time interval between germination and the formation of new sporangium. It is interesting to note that haploid spores are twice as sensitive as diploids.

The development of maximum resistance to cycloheximide is related to the concentration of the antibiotic in which the fungus is grown and, to maintain this resistance, the continued presence of the antibiotic is essential. The degree of tolerance of spores is directly related to the mycelium producing them, and it can be shown that they will develop in concentrations at least equal to those withstood by the parent spores. When either spores or mycelium are exposed to concentration of antibiotic incompatible with growth they are killed, but killed slowly, since 50% are still viable at 12 hours.

M.-L. J.

ANATOMY SEMINAR

April 21, 1953

PASSAGE OF TUMOR CELL EMBOLI IN PULMONARY CIRCULATION AND LYMPHATIC SYSTEM. By Irving Zeider, New England Deaconess Hospital, Boston, Massachusetts.

Rabbits and rats received intravenous injections of rabbit and rat tumor cell suspensions. Immediately after injection a sample of aortic blood was collected and re-injected into normal animals and there observed for appearance of tumor. Of the two rabbit carcinomas used, the Brown-Pearce strain gave 50 per cent positive results, with tumor appearing in many organs, while the V_2 strain had a considerably lower rate of immediate passage through the pulmonary circulation. This observation is compatible with the natural history of these tumors since the Brown-Pearce usually metastasizes to many organs beyond the lung; the spread of V_2 is practically limited to the lung itself.

Lymph node metastases were studied by injecting (1) fixed and stained, and (2) viable cancer-cell suspensions into afferent lymphatics. In both, tumor cells were found localized in the subcapsular lymph sinuses; this is also the case in humans. When the lymph nodes in the path of the tumor cells were successively excised at varying intervals after the injection, it was found that the later the primarily involved node was removed, the more frequently metastases occurred to central sites; if the primary node was removed in one to three days, there was no extension beyond it. Before metastasis occurred centrally along a lymphatic chain more than 95 per cent of the primarily involved node's substance was replaced by tumor. Intra-

vascular extension may occur in lymph nodes, however, and extensive metastases are seen without progressive lymphatic transmission.

N. A. H.

ANATOMY SEMINAR

May 5, 1953

EMBRYOLOGICAL DEVELOPMENT OF NEUROSECRETORY MATERIAL UNDER NORMAL AND PATHOLOGICAL CONDITIONS. By K. Benirschke, Free Hospital for Women, Brookline, Massachusetts.

The posterior pituitary and hypothalamus of the human fetus have been subjects of study with regard to the development of neurosecretory material. Gomori's method was applied to tissue obtained from therapeutic abortions and stillbirths, starting with fetuses of 10 weeks' gestation. Blue granules, representing neurosecretory material, first appeared at 20 weeks, located in the ganglion cells of the supraoptic and paraventricular nuclei. Dustlike granulations along the fibers in the anterior part of the posterior lobe of the pituitary developed at 23 weeks followed by occasional Herring bodies at 34 weeks. At birth, Herring bodies were present along with numerous granulations around the vessels of the anterior and posterior parts of the neurohypophysis.

Several cases of renal agenesis with oligohydramnios revealed increased amounts of the granular material. In a more carefully observed group of anencephalic monsters with hydramnios, however, there was no granular material in the posterior lobes and only slight granular material of questionable origin in the supraoptic and paraventricular nuclei. Nissl substance was also absent. The humoral nature of these granulations is not, of course, established, but it is suggested that the hydramnios and dilatation of the urinary tract associated with anencephaly might be explained on the basis of deficient antidiuretic hormone production.

O. L. K.

YALE MEDICAL SOCIETY

Presentation of M.D. Theses by Senior Medical Students

May 13, 1953

FATE AND MICROSCOPIC STUDY OF A PEDICLE SKIN GRAFT IN THE TRACHEA OF DOGS. By William A. Whalen, Jr.

The problem of cicatricial stenosis in the larynx and trachea has been one of the most perplexing problems confronting the otolaryngologist for many years. The difficulty stems from the fact that surgical excision of a stenosed area in the lumen of the larynx or trachea results in early recurrence of the stenosis unless a suitable lining for the lumen is provided at the time of operation.

The successful use of skin grafts in treatment of stenosis of the larynx was originally introduced by Arbuckle in 1928. Many investigators since have transplanted skin for laryngeal repair, but there have been no reports concerning the structural status of successful skin grafts growing under a respiratory environment after given periods of growth. Therefore, the microscopic study of successful grafts was undertaken and is herewith reported.

Pedicle skin flaps were raised from the necks of four dogs and transplanted into their tracheas. When the grafts had taken, the flaps were severed from their external blood supply, and tracheofissures were closed

with the skin transplants within the tracheas. Skin graft specimens were obtained at three, four, five, and six months following this procedure.

Microscopic study revealed that all of the grafts were intact with preservation of the stratum granulosum and stratum germinativum except for one specimen in which half of the graft sloughed away; re-epithelialization of pseudostratified ciliated columnar type occurred over the denuded area. The stratum corneum had sloughed in all specimens. Areas of metaplasia from tracheal epithelium to stratified squamous epithelium were noted adjacent to the grafted areas; past investigation has shown that skin grafting to the buccal and nasal mucosa has resulted in metaplasia to the cell type of the graft site.

From a review of the literature, it seems evident that split thickness skin grafting in the larynx is more effective than pedicle full thickness grafting. One of the major disadvantages of using any skin as the lining material is that the sebaceous glands of the skin secrete tenacious mucus on the surface of the graft with resultant formation of foul smelling crusts. Tissue which lacks secretory function, such as fascia lata, would be a good substitute for laryngeal skin grafting. Clinical trial with fascial grafts has met with poor results in the oral cavity; tracheal reconstruction with fascia lata, however, has proved quite successful. Finally, recent work on the removal of the epidermis of skin and the utilization of the dermis for grafting may prove this to be the tissue most acceptable surgically.

WILLIAM A. WHALEN, JR.

THE MECHANISM AND BIOLOGICAL SIGNIFICANCE OF ENZYMATIC TRANSPEPTIDATION. By Jack Durell.

Two problems face the biochemist in his attempt to analyze the mechanism of the biosynthesis of proteins: how is the energy of peptide synthesis provided, and how is the marked individuality of each specific protein molecule achieved? For almost 75 years various investigators have considered the possibility that proteolytic enzymes participated in the biosynthesis of proteins; in recent years, with the growing knowledge of the specificity of proteolytic enzymes and the wide spectrum of intracellular proteinases, the hypothesis that these enzymes catalyze protein synthesis has become more attractive. Previously, only a mass action reversal of hydrolysis was considered in ascribing synthetic functions to proteinases; the formation of plastein and the synthesis of anilides have been considered supportive evidence. Recently, attention has been turned to transfer reactions linked with specific synthetic reactions to result in the synthesis of specific polypeptides. Glutamine and glutathione, two compounds whose mechanisms of synthesis have been studied, have been suggested as possible starting materials for transpeptidation. To date, every proteolytic enzyme studied for ability to catalyze transamidation reactions has given positive results with appropriate substrates and nucleophilic replacement agents.

Recently, we have developed a kinetic equation based on a mechanism of transamidation assuming a competition between water and the replacement agent for the activated bond. For the explicit solution of the equation, two constants must be evaluated for each system studied; when this is done, the equation is found to predict the experimental results. One of the constants, C, is really a direct measure of the efficiency of transamidation. Two enzymes, papain and trypsin, have been compared for the efficiency of transamidation of the substrate benzoyl-L-arginylamide, a suitable substrate for both enzymes, with hydroxylamine. In view of the suggestion that intra-

cellular proteinases participate in protein synthesis via catalyzing transamidation, it is interesting that the intracellular enzyme preparation, papain, was found to be fifteen times more efficient in transamidation than the digestive enzyme, trypsin. The evidence at present is only suggestive; much remains to be discovered before a rôle for transpeptidation in protein synthesis can be asserted.

J. DURELL

TRANSPLANTATION OF FREEZE-DRIED AORTIC HOMOGRAPHS IN DOGS WITH REFERENCE TO ACUTE HYPERVOLEMIA AND LIPEMIA. By T. O. Gentsch.

Freeze-dried aortic homografts which had been stored for an unlimited time at room temperature were implanted in the thoracic aortas of fourteen normal dogs. Of the ten dogs which survived the operation, eight showed successful function after ten weeks. The other two dogs died from an infection at the graft site.

An attempt to evaluate the ultimate fate of the graphs was made by subjecting four of the eight dogs with successful implants to a controlled acute lipemia stress. These dogs received serial rapid intravenous infusions of 17 cc./kilogram of body weight of a 2:1 egg yolk-saline mixture. After ten days of infusions, the surviving animals were sacrificed and examined.

The grafts appeared to be markedly more susceptible to the degenerative changes of hypervolemia and lipemia than did the adjacent normal aorta. These changes were in the form of an active inflammatory reaction, a diffuse suffusion of fat with fat-filled foam cells especially in the intima, a diffuse medial calcification in half of the stressed grafts, and a medial hemorrhage in one graft.

T. O. GENTSCH

THE EFFECT OF WATER LOADING ON THE ANTIDIURESIS OF THE PASSIVE ERECT POSTURE. By Irving H. Goldberg.

Once the state of "physiological diabetes insipidus" has been maintained in normal subjects in the recumbent posture, further increase in the dilution of body fluids does not augment urine flow. If a subject in this state of physiological diabetes insipidus is tilted into the passive erect posture (PEP), an antidiuresis and antisaluresis will ensue. It seems clear that part of the antidiuresis must be related to the secretion of the antidiuretic hormone (ADH) of the posterior pituitary gland. The nature of the stimulus that promotes the ultimate antidiuresis and antisaluresis in the PEP is not well defined, but it seems to be due to a "contracted volume" in some key part of the body fluids, probably some part of the vascular system.

This investigation was undertaken to learn whether dilution of the body fluids beyond that necessary to promote complete suppression of secretion of ADH in recumbency would modify the degree of antidiuresis in the PEP.

The effects of water loading on the rates of excretion of water and electrolytes were studied in normal young adult males in the recumbent position and in the PEP (the subject tilted to an angle of 15 degrees with horizontal, with head raised up and lower limbs completely relaxed.) Two studies were performed on each subject, one in which the subject received a small water load and another in which he received a large water load. By increasing the load of water in the body beyond 1 per cent of body water, the percentage of maximum antidiuresis was decreased by increasing

the load of water in the body. The modifying effect of the larger water load was not due to its influence on glomerular filtration rate, nor do calculations of the increment of reabsorbate due to PEP and the clearance of free water imply that the modifying effect of the larger water load was due to any effect on proximal tubular solute reabsorption. Moreover, while the differences in percentage antidiuresis associated with the PEP in the paired studies were statistically significant, the differences in percentage of antisaluresis were not. It is therefore difficult to explain the difference in the degree of antidiuresis on the associated changes in salt if these latter changes were not significantly different in the small and large water loads. Differences in degree of antidiuresis associated with the small and larger water loads were not due to changes in plasma volume. There was no evidence that plasma volume was better supported by the larger than by the smaller water loads. There was a greater degree of hypotoxicity with the large as compared with the small loads in every instance, both in percentage change and in actual magnitude. It is suggested that hypotoxicity of the body fluids may be the influence which modified the response of the posterior pituitary gland to the PEP.

IRVING H. GOLDBERG

ALPHA OMEGA ALPHA LECTURE

May 19, 1953

THE METABOLISM OF HEMOGLOBIN AND BILE PIGMENT. By Irving M. London, Department of Medicine, College of Physicians and Surgeons, Columbia University, New York, New York.

The feeding of labeled glycine to man reveals that isotopic nitrogen, N^{15}, is incorporated into the heme of normal circulating red blood cells. The disappearance of N^{15} from such cells corresponds to the normal life cycle of the red cell. This observation suggests that normal erythrocyte destruction is not a random process. Clinical application of the method indicates that the basic defect in polycythemia vera is the over-production of cells with a normal life span, while in hemolytic anemias such as sickle cell anemia and in pernicious anemia the early exponential fall of N^{15} heme concentration points to an indiscriminate destruction of red cells by the body.

A valuable addition to methods for the study of heme has resulted from the finding that labeled heme may be synthesized *in vitro* from glycine N^{15} by erythrocytes. Blood containing many reticulocytes, such as that of birds, is especially suitable for these *in vitro* studies. It has been shown, for example, that lead inhibits the synthesis of heme in this system.

Finally, the incorporation of isotopes in stercobilin has been studied. N^{15} appears in stercobilin immediately after the feeding of isotopic glycine and before the destruction of circulating erythrocytes has occurred, indicating that stercobilin does not all come from the breakdown of normal red cells. In the normal, this fraction constitutes approximately 11 per cent of total stercobilin excretion. Especially large amounts of this fraction are seen, however, in pernicious anemia and congenital porphyria, substantiating the hypothesis that all bile pigments do not come from the red cell. Injected heme N^{15} appears as labeled stercobilin but not as hemoglobin N^{15}. Similarly, injected protoporphyrin containing $carbon^{14}$ appears in stercobilin. It has therefore been suggested that bile pigments are derived not only from hemoglobin, but from certain hemoglobin precursors as well.

O. L. K.

BOOK REVIEWS

A HISTORY OF SCIENCE. By George Sarton. Cambridge, Harvard University Press, 1952. 646 pp. $10.00.

George Sarton is, in many ways, a twentieth century example of the Renaissance ideal of the "universal man." In our age of specialization it is almost impossible to attach a conventional label to Dr. Sarton. He is a scientist who received the degree of doctor of science in mathematics from the University of Ghent; he is a novelist (*La chaine d'or*, 1909, written under the pseudonym of Dominique de Bray); he is a linguist whose knowledge of ancient and Eastern languages would put a Renaissance humanist to shame; and, above all, he is a historian of science who has, almost single-handed, raised this study from the status of a hobby among scientists to a full-fledged and recognized scholarly discipline.

His activities in the history of science alone invite wonder from his colleagues. He has been one of the most effective propagandists for this new field in *The history of science and the new humanism* and a host of journal articles. He founded (1912) and supported out of his own pocket, *Isis*, the first journal devoted to the history of science, followed in later years by two other journals, *Osiris* (1936) and *Horus* (1952). Finally, in the years 1927 to 1948, he produced his monumental *Introduction to the history of science*, which lists and discusses all known persons who engaged in scientific activity or speculation from the dawn of history to the end of the fourteenth century. This work is to the historian of science what the *Handbook of chemistry and physics* is to the physical scientist—an indispensable tool but not a work which can be read to gain a comprehensive view of the field.

Professor Sarton has long realized the need for such a comprehensive view, and his projected eight-volume history of science is intended to provide this. If the first volume on ancient science is representative of the volumes to follow, then Professor Sarton will have succeeded admirably. This is no narrow history of science reciting the names of scientists and philosophers and their contributions to the history of thought. It is, rather, a cultural history in which science occupies the central position. Thus Professor Sarton has attempted to describe the civilizations of ancient Egypt, Mesopotamia, and Greece, discussing not only their scientific work but the place of science in these cultures and the action of other cultural forces on the development of the sciences. Unfortunately, the attempt is not always successful. The short sections on the political history of Egypt, Mesopotamia, and even Greece are necessarily so brief that they contribute little to the later sections on science. A graver fault, in the reviewer's opinion, is the neglect of the influence of economic and social conditions on the growth of science. This criticism applies particularly to Greece, which Professor Sarton tends to see solely as a land of beauty and intellect disturbed by unfortunate wars. But what of economic conditions? What of the social classes? Did not the existence of an intellectual and social aristocracy with few contacts with the manual arts have some influence on the speculative and philosophical nature of Hellenic physical science?

But these, after all, are minor criticisms. Considering the scope of this volume, one can only admire the competence with which it has been written. The wit and ease of reading reflect George Sarton the novelist, the technical

discussions show us George Sarton the scientist, and the whole book reflects the profound humanism of George Sarton the scholar. This volume, and the seven to follow, will give us a history of science which will be an essential part of the library of the scientist, the historian, and the intelligent layman.

PEARCE WILLIAMS

THE APOLOGIE AND TREATISE OF AMBROISE PARÉ CONTAINING THE VOYAGES MADE INTO DIVERS PLACES, WITH MANY OF HIS WRITINGS UPON SURGERY. Geoffrey Keynes, Ed. Chicago, University of Chicago Press, 1952. xxv + 227 pp., 4 plates. $3.50.

Ambroise Paré was born the son of a French barber and received no education before he entered the field of surgery other than an apprenticeship to a local barber-surgeon. By careful observation and much thought, tinged with scepticism, about what he observed, he rose to the relatively exalted position of Fellow of the Collège de St. Côme and personal surgeon to four successive French kings. Although he cannot be credited with being the first to advocate the use of ligature, he is doubtless the one man responsible for introducing the routine use of it in amputations, in place of cauterization, for the control of hemorrhage. His *Apologie and Treatise* was written in answer to an attack by Étienne Gourmelon, Dean of the medical faculty of the University of Paris and Professor of Surgery, which aimed to discredit Paré and his methods. It gives us a picture of his personality as well as of the medical and political world in which he lived and shows Paré to be an interesting, sometimes engaging, writer who only occasionally becomes tiresome by dwelling overlong on irrelevant details.

The translation used is that of the Thomas Johnson folio of 1634, which is known to contain translational inaccuracies. One wishes Mr. Keynes had provided a glossary with the text. This would certainly be useful for the reader unfamiliar with sixteenth century anatomical and medical terminology.

In any selective anthology there is bound to be some disagreement about selections excluded, as well as those included. I would have been pleased to find excerpts from Paré's description of the Plague, as well as some part of his description of the podalic version.

The book is well indexed and contains an adequate review of Paré's life, as well as a listing of his major publications. There are collotypes of four contemporary portraits and a short bibliography. The paper and binding are of moderately good quality.

Though this book leaves something to be desired as an academic contribution, the works contained herein speak for themselves and require no other justification for their inclusion in any collection of the history of medicine.

J. M. Q.

DYNAMIC PSYCHIATRY. Franz Alexander and Helen Ross, Eds. Chicago, University of Chicago Press, 1952. 578 pp. $10.

Somewhere in this book one of the contributors remarks: "Up to now there has been no generally accepted, formal textbook of psychoanalytic psychiatry . . . ," implying, perhaps, that the present volume will be that textbook. Indeed, the prepublication rumors suggested that this would be its function. Because there is such a clear need for a counterpart in modern psychiatry of reliable texts in other fields such as Cecil's and Harrison's in

medicine, it was almost inevitable that this wishful idea would arise. Unfortunately, this is not the much-wanted definitive text. This feeling of disappointment in the book, however, is not due to any failure to attain the aims stated in the preface: "This volume offers to students of psychiatry a comprehensive view of dynamic psychiatry. The dynamic trend in psychiatry is the result of the impact of psychoanalysis, its theory, its method of investigation, and its therapy, upon the whole of psychiatry." This goal is, by and large, admirably attained.

The book is composed of 16 chapters subdivided into three parts dealing with fundamental concepts, clinical psychiatry, and the influence of psychoanalysis on allied fields. The scheme is that each of the chapters is written by an individual with special interest in the topic discussed. In the early chapters, especially, there is insufficient editing. This is reflected in some tedious repetition, lack of integration, and inappropriate emphases. In Part I, Benedek's essay on Personality Development deserves special praise. To a subject which invites pedantry and obfuscation she has brought refreshing, new, and stimulating viewpoints. The chapters in the section on clinical psychiatry satisfactorily fulfill the general aim of the book and with a few exceptions are scholarly in tone. Whitehorn's article on the psychoses particularly warrants this adjective. In a volume of this size one seems justified in raising the question of the advisability of donating so much space (25 pages) to Acute Neurotic Reactions, especially when the remainder of neurotic disorders is covered in only 33 pages.

In this reviewer's opinion, there are two chapters in the clinical psychiatry section which are of such a high level of excellence as to make the volume worth owning just to have them convenient for reference. One is Levine's article on Psychiatric Treatment. With great clarity of style, he offers a tangible means of understanding the aims and levels of psychotherapy. Although it might not be satisfactory to well-advanced psychoanalytic students, it is eminently so for the beginner and the intermediate. The other outstanding chapter is Brosin's Contributions of Psychoanalysis to the Study of Organic Cerebral Disorders. This is an absolutely first-rate contribution. In the first place, it provides a unified collection of information about hypotheses and researches previously obtainable only through laborious library work. In the second place, the author has offered interpretations of data, criticisms, and suggestions for future research which are exhilarating in their originality.

The third part of the book makes pleasant, informative, "after-work" reading. As is to be expected, there is an over-all expansiveness of tone, but usually modulated by the obvious authoritativeness of some of the basic ideas.

The individual bibliographies of the chapters are ordinarily excellent, especially those for the organic cerebral disorders and the influence of psychoanalysis on current thought.

JOHN W. HIGGINS

DYNAMIC ASPECTS OF BIOCHEMISTRY. By Ernest Baldwin. 2d ed. New York, Cambridge University Press, 1952. 494 pp. $5.00.

The appearance of the second edition of this lucidly written and aptly titled volume is a happy event for students of biochemistry, biology, or related fields. As in the first edition, Professor Baldwin has been able to interweave the factual content of biochemistry into a dynamic narrative

which creates such interest that the student is unaware of the process of assimilation of facts.

The volume is similar to the first edition in form, consisting of two broad sections which discuss the static and the dynamic aspects of biochemistry. Since a division on this basis cannot be mutually exclusive, the overlap serves the pedagogically desirable principles of repetition, emphasis, and integration.

The first part consists of a greatly expanded discusion of substrates, enzymes, coenzymes, and mechanisms of interaction of these classes of substances. It has been considerably improved by a chapter on "Biological Energetics" which this reviewer particularly enjoyed and which fulfills a long-standing need. The author's discussion of the concepts of free energy and entropy has managed to avoid the lengthy thermodynamic arguments put forward in more rigorous treatments without slipping into the type of loose thinking which has characterized many such discussions in earlier texts for non-specialists. The application of energetic concepts to biosynthesis is particularly well done.

The second part, entitled "Metabolism," has been expanded and brought up to date as far as is possible for a subject which is in such a state of flux. The incorporation of the many recent discoveries bearing on the citric acid cycle and on fat metabolism, particularly the relationship of acetyl coenzyme A to these processes, has been well done and should be a boon to investigators in related fields who have not had the opportunity to familiarize themselves with this knowledge or integrate it into a more complete picture of metabolism.

This reviewer feels that the somewhat weak discussion of the synthesis, degradation, and interrelationships of the amino acids, which characterized the first edition, might have been reinforced in the second. However, the question of emphasis is a subjective matter and accordingly the author's decision and treatment cannot be regarded as fault in a book which possesses so many virtues.

To summarize the reviewer's opinions without indulging in the usual clichés is a difficult task which is best accomplished by stating that the volume is recommended to the neophyte as a fascinating introduction and to the specialist in a related field as a painless way of integrating a diversified field. As the author states in the preface, the book is directed to a particular class of readers, and for this class, it is unusually well suited. The book, while adequately documented, is not an encyclopedical collection of facts and, accordingly, the person seeking a comprehensive work should turn elsewhere.

PETER S. CAMMARATA

PROBLEMS OF CONSCIOUSNESS. Transactions of the Third Conference, March 10 and 11, 1952. Harold A. Abramson, Ed. New York, Josiah Macy, Jr., Foundation, 1952. 156 pp. $3.25.

Presentations by three members of the conference and the "Group Interchanges" provoked thereby make up this latest publication of the Macy conferences on consciousness.

The effects of convulsions, coma, psychoses, and several states of endocrine imbalance upon cerebral circulation and metabolism are presented by Seymour S. Kety. Dr. Kety parenthetically states his belief that behavior, but not consciousness, may be explained by a completely mechanistic theory.

The controversy that results seems to stem not from different definitions of "consciousness," as some of the participants suggest, but rather from a lack of agreement upon the meaning of "explain." Transcripts of a hypnoanalytic treatment session and of Beck's movie, "Unconscious Motivation," are used by Lewis R. Wolberg to illustrate "Hypnotic Phenomena." Marcel Monnier summarizes the changes in EEG pattern that correspond to different states of consciousness and attention, and outlines the antagonistic subcortical systems that regulate consciousness.

This book shares the virtues and faults of the previous volumes of this series. The papers are interesting, but most of the data reported are available elsewhere; the discussions likewise are interesting, but not so full of brilliant ideas as to demand verbatim publication.

W. A. WILSON, JR.

ALCOHOLISM 1941-1951. A Survey of Activities in Research, Education and Therapy. By the Staff of the Yale Center of Alcohol Studies. New Haven, Quarterly Journal of Studies on Alcohol, 1952. 511 pp. $1.00.

So many groups and disciplines (scientific and otherwise) have become involved to varying degrees in the problems of alcoholism that an attempt to survey their activities is exceedingly difficult. For the most part, this survey fulfills this aim and provides a convenient way of looking over the recent past to examine the accomplishments of various fields of physiological and sociological research and educational programs. Those with special interests will probably discover various examples of the inevitable omissions and particular emphases which will stimulate negative criticism. Although one's general response is of appreciation for the effort expended in constructing this summary, it is difficult to resist a complaint about the complete absence of any bibliography. Although the reasons for the omission are given in the Introduction, there are several places where the lack is sorely felt. For example, Lester and Greenberg give an excellent summary of physiological research, but leave the reader only partially satisfied, because of the absence of references to the original work.

To offset this fault, however, there are full directions in one of the articles as to how to make use of a master bibliography on alcoholism accumulated by the Yale Center of Alcohol Studies (which service, incidentally, is a major contribution). One might wish that the energy and organizational technique directed toward alcoholism as depicted in this volume could have their counterparts in the general field of mental illnesses.

JOHN W. HIGGINS

THE RIDDLE OF CANCER. 2d ed. By Charles Oberling. William H. Woglem, Trans. New Haven, Yale University Press. xii + 238 pp. $5.00.

Cancer—"its name has a mysterious and disquieting sound, and no one has ever really understood why Hippocrates chose it." The above statement reveals consideration of sociopsychological aspects, the antiquity of the disease that still challenges the scientific ingenuities of man, and indicates the scope of this short volume.

Although seemingly formidable with its extensive bibliography, the book is surprisingly easy to read. It is well written and well organized. In a few paragraphs the story of Fibiger's quest of the etiology of the rat's gastric tumor reveals the struggles and disappointments of cancer research. The

accuracy of statement is also commendable for so concentrated a treatment. The book is about 30 pages longer than the first edition. Two significant new sections have been added: General Principles of Chemical Carcinogenesis and Some Objections to the Virus Theory. The major additional pages treat new studies on the causes of cancer.

Those who are primarily concerned with the metabolism and treatment of cancer may be disappointed that their interests are not represented. Even to those, however, the book should be of interest; well related and organized information on the causes of cancer must always be of value. A commendable book.

W. U. GARDNER

THE FIGHT AGAINST TUBERCULOSIS: AN AUTOBIOGRAPHY. By Francis Marion Pottenger. New York, Henry Schuman, Inc., 1952. xii + 276 pp. $4.00.

Some years ago Dr. Pottenger wrote a fairly complete story of his life and gave each of his children a copy for Christmas. Friends who read it told him it should be preserved in a more permanent form, and this book is the result. He describes his boyhood on an Ohio farm, where an early interest in nature fostered by the family physician turned him to medicine. After an old-style medical education and a trip to sit at the feet of German and Austrian masters, he returned to Cincinnati to practise and teach. His wife's tuberculosis, however, took them to California, where her death in 1898 determined him to specialize in this disease. In 1902 he founded the Southern California Antituberculosis League, the first in the West, and in 1903 established one of the state's earliest sanatoriums. The rest of the book covers various aspects of his personal life and career, including the discovery of new diagnostic signs in diseases of the chest, later trips to Europe, and teaching activities.

The course of Dr. Pottenger's fruitful life has the makings of an interesting story, but his style of writing, though clear, is without grace. The accounts of his trips to Europe consist largely of long lists of important people he met and places he saw, each with a brief comment or description, composed without that literary imagination and ability necessary to make a traveler's account more than a device for jogging the author's memory. The book is poorly organized, full of dogmatic assertions, and replete with dull details. There are errors of fact. The German Revolution of 1848, for example, was not "Socialist" but Liberal (p. 68). Tuberculosis was made reportable in New York City in 1897, not 1907 (pp. 118-119). More important is his strong implication that the preventive and curative measures of medicine have alone been responsible for the decline of the tuberculosis mortality rate since 1900 (pp. vii, 225). Even those measures, moreover, have not been adequately described. *The Fight Against Tuberculosis* is hardly an accurate title for this compilation of personal reminiscences and miscellaneous notes.

JOHN B. BLAKE

MICROBIOLOGY OF WATER AND SEWAGE. By P. L. Gainey and Thomas H. Lord. New York, Prentice-Hall, Inc., 1952. 404 pp. $5.90.

This volume and its laboratory manual are prepared as guides in an undergraduate course at Kansas State College in the microbiology of water and sewage. Necessary information is included for an understanding of the

principles underlying phenomena encountered in observation of water supplies and sewerage or sewage disposal systems as they affect public health. For individuals with relatively little knowledge of bacteriology this book will be especially useful. It will be helpful also as a reference concerning water treatment.

ERIC W. MOOD

THE DESIGN AND ANALYSIS OF EXPERIMENTS. By Oscar Kempthorne. New York, John Wiley and Sons, Inc., 1952. 631 pp. $8.50.

This book by a teacher of statistics (as well as a consultant for "experimenters") is a comprehensive study of the philosophical background for the statistical design of experiment. It is necessary to have some facility with algebraic notation and manipulation to be able to use the volume intelligently. The problems are presented from the theoretical point of view, without such practical examples as would be helpful for those not acquainted with mathematics. The mathematical justification for the techniques is given.

As a somewhat advanced treatment of the design and analysis of experiments, this volume will be interesting and helpful for many who approach statistics theoretically as well as practically. With emphasis on the "why," and with description given broadly, the author relates the subject matter to the general theory of statistics and to the general problem of experimental inference.

MARGARET J. ROBERTSON

BIOCHEMICAL PREPARATIONS, Vol. 2. Eric G. Ball, Ed., New York, John Wiley and Sons, Inc., 1952. vii + 109 pp. $3.00.

Volume 2 of *Biochemical preparations* continues to bring to the investigator of biological materials reliable methods of preparing substances useful in his research. E. G. Ball is the editor-in-chief of this volume, having taken over after the death of R. C. Sealock. The present volume follows the format established in Volume 1 of this series, in which the following aspects of each preparation are considered: (i) the principle employed in the preparation, (ii) the nature and purity of the starting material, (iii) a clear, detailed description of the procedure employed, (iv) the properties and purity of the product obtained by the described procedure; and (v) alternate methods of preparation of the material, with the appropriate references. As has been the practice in *Organic syntheses* and in the previous volume of *Biochemical preparations*, each preparation has been checked by an independent laboratory. The details of each preparation are presented so explicitly that even the novice should be able to carry them out with minimum difficulty.

The procedures which may interest the researcher in the area of proteins and amino acids are as follows: L-aspartic acid is prepared by the hydrolysis of L-asparagine, and DL-glutamic acid by the racemization of L-glutamic acid. The carbobenzoxy method is described for the preparation of glutathione. The procedures for the isolation of cucurbit seed globulin and the sodium salt of phosvitin are presented, also the method of oxidation of casein to oxycasein and for the isolation of cyctochrome C and crystalline lactate dehydrogenase.

The increasing use of spectraphotometric methods for enzyme assay of diphosphopyridine nucleotide-linked enzymes will give the procedure for reduction and isolation of reduced diphosphopyridine nucleotide considerable value.

In the area of the biochemistry of the carbohydrates the syntheses of glucose-6-phosphate and phosphoryl-enolpyruvic acid are described. Myo- and DL-epi-inosose-2 are prepared by the microbiological oxidation of inositol. The enzymatic degradation of sodium phytate is employed for the preparation of inositol monophosphate. In addition a method for the purification of commercial fructose-1,6-diphosphate and a method for the preparation of sodium pyruvate by neutralization of pyruvic acid are presented, also a method for obtaining uniformly labeled C-14 sucrose by photosynthesis.

Procedures which may interest investigators concerned with the lipids are the isolation of oleic acid, the syntheses of the methyl esters of oleic and ricinoleic acids, and the syntheses of phosphocholine and α-glycerolphosphoric acid.

The index is cumulative for volumes 1 and 2, and the preface contains a list of compounds of biochemical interest which have appeared in *Organic syntheses* through Volume 27.

With few exceptions the material is presented in an excellent manner. However, the basic value of a book of this type should be to give methods of preparing materials which are in demand and for which no good alternative source is available. A number of the preparations in this volume do not meet these requirements. For example, much of the book is devoted to the synthesis of glutathione. This is available commercially, and the method given is tedious and not the most practical way of obtaining glutathione. Furthermore, there is no reference to the more recent synthetic method of Hegedus (Helv. Chim. Acta., *31*, 47). Notably absent are methods of preparation of enzymes. Cytochrome C and crystalline lactic dehydrogenase are the only purified enzymes which are included.

Although this volume does not make the contribution that it could to current biochemical methodology, it will be of considerable value to persons engaged in biochemical research and to teachers of advanced courses in practical biochemistry.

ROBERT B. JOHNSTON

DOCTORS IN BLUE. By George Worthington Adams. New York, Henry Schuman, 1952. 245 pp., illus. $4.00.

Many military historians consider the American Civil War the first "modern" war. It is apt then that the rise of modern military medicine as we now know it dates also from the Civil War. Many of the "inventions" of military medicine had existed in other armies, but in scattered or fragmentary form. The present distribution and use of ambulances, the permanent allotment of personnel to medical service units, the distribution and function of field hospital units, the divided assignments of physicians in the field, these are some of the organizational aspects of military medicine for which we are indebted to the Union Medical Corps. Indeed, some of these systems, notably those regarding ambulances and field hospitals, have been copied by many other armies and are in use today in a form little changed.

Not as much was contributed to medicine itself, however. Some excellent work was done by individuals working within a specialized field. Outstand-

ing examples are Mitchell and Keen in neurosurgery. The average army surgeon was sent back to civilian practice a better surgeon, with an increased respect for cleanliness and ventilation of the sick-room. Women who "nursed" the wounded began to feel the need for professional training. But these were advances within the individual and not within the body of medicine as a whole.

Dr. Adams has done an excellent piece of work in compressing into this one volume the mass of material available. The book abounds with fascinating bits of information, such as Surgeon-General Hammond's order to discontinue the use of calomel and tartar emetic and the uproar that this created among the Army physicians. Today we are accustomed to an adequate pre-induction physical examination for the draftee. Then, however, as the author points out, the examinations were so poorly or hastily conducted that approximately four hundred women were inducted! Physicians of today who feel that the Army physical standards are too low would recoil from the Civil War examiner who passed, with official sanction, incipient tuberculosis and secondary syphilis.

The author discusses his topic under the general headings of "Confusions of 1861," "Beginnings of an Ambulance System," "Wartime Surgery," "Nurses, Staffs, and Convalescents," "Diseases and Treatments," etc. There are eight pages of excellent photographs of hospitals, ambulances, and staff. For the statistically minded or the comparative reseacher there is a comprehensive index, which tabulates wounds, diseases, etc., by place, race, date, and kind, with comparisons between Union and Confederate where possible.

In writing a book of this kind, an author is faced with two types of internal organization. Either he may develop each theme separately, returning to the beginning of the war with each new topic, or he may develop the book chronologically and discuss all his topics at once, year by year. One wishes the author had made a choice. Instead he has a tendency to pick and choose his dates and chronology, with no system at all. The reader finds Surgeon-General Hammond continually being discharged, goes through first and second Manassas several times, and is kept in a state of confusion about place and date without ever being sure of where Dr. Adams is going to carry him next. For the general reader, unfamiliar with the War in detail, more discussion of purely military history would have been helpful, perhaps in a page at the beginning of each new section.

However, Dr. Adams has supplied us with a splendid account of the Union medical forces in a book that must be on the shelves of every amateur in medical or Civil War history. I hope that he will soon publish a companion volume about the gallant gentlemen in grey.

ROBERT J. T. JOY

MORBUS ALZHEIMER AND MORBUS PICK. A Genetic, Clinical and Pathoanatomical Study. By Torsten Sjogren, Hakon Sjogren, and Ake G. H. Lindgren. Acta Psychiatrica et Neurologica Scandinavica Supplementum 82. Copenhagen, Ejnar Munksgaard, 1952. 150 pp.

This monograph presents the genetic (T. Sjogren), clinical (H. Sjogren), and pathological (A. Lindgren) findings in a group of well-studied cases of the Pick and Alzheimer syndromes. The material consists of 80 probands, 58 females, 22 males. The brains of 36 probands have been histo-pathologically examined, 18 falling into the Alzheimer (Group I)

and 18 into the Pick group (Group II). Twenty-nine cases (Group III) were not histologically verified but demonstrated cerebral atrophy by air-encephalography (24 cases) or gross post-mortem examination of the brain (five cases). Group IV consists of 15 cases with clinical diagnosis of Pick-Alzheimer syndrome but lacking radiological or autopsy confirmation.

Average age of onset, duration of illness, and age at death for each group respectively:

Group I: 53.1-7.1 and 60.2 years
Group II: 54.5-6.7 and 61.2 years
Group III: 56.4-5.7 and 62.4 years
Group IV: 56.1-7.8 and 65.2 years

Genetic statistical analysis of the material supports the general opinion that the morbidity rate for parents and siblings is higher in Pick's disease than in the Alzheimer group. The Alzheimer-Pick syndromes represent 10 per cent of all presenile and senile psychoses in Sweden.

Survey of the clinical signs and symptoms in this material is a useful and succinct summary of the characteristics of these presenile dementias. Considerable space is given to possible anatomical substrates of symptomatology. The impossibility of differentiating the Pick and Alzheimer syndromes on clinical grounds alone is emphasized. However, several differential points of significance in the authors' histologically verified cases are of interest. There was a high frequency of gait and the tone disturbance in the Alzheimer group (akinetic hypertonia with or without cogwheel phenomenon or rigidity) and a rarity of these in the Pick group. Early spatial disorientation was also highly characteristic of the Alzheimer cases. Aphasic disturbances were nearly 100 per cent in both groups. Pathologically, the Alzheimer cases displayed a somewhat diffuse gross atrophy maximally affecting the frontal and temporal regions. Histologically, the pathognomonic fibrillary nerve cell changes and senile plaques were universally and abundantly present. Basal ganglia and midbrain nuclear structures were commonly involved in the Alzheimer cases only.

In the Pick group, gross atrophy was more obvious and limited more sharply to the frontal and temporal lobes. Fibrillary changes and plaques were universally absent, while "balloon" nerve cells in the cortex and argentophilic granules in cytoplasm of neurones were regularly found. Arteriosclerotic changes were on the whole strikingly absent in the brains of both groups, particularly the Pick material.

An extensive bibliography and some excellent illustrations are included.

JANICE STEVENS

PROGRESS IN BIOPHYSICS AND BIOPHYSICAL CHEMISTRY, Vol. 2. J. A. V. Butler and J. T. Randall, Eds. New York, Academic Press, 1951. 300 pp. $6.80.

The field of biophysics in these days is breathless from rapid growth, harassed by the number of journals publishing relevant material, and perplexed by the vagueness of the boundaries between itself and its many sister sciences. The appearance of such books as this, with its review articles by acknowledged authorities in the eight fields covered, will be an important factor in constructing a common core of knowledge for the field. May there be more such books in the future.

This reviewer was particularly impressed by "The Biophysical Aspects of Polysaccharide Structure in Plants" by R. D. Preston and by "Quantum

Physics of Vision: Theoretical Discussion" by M. H. Pirenne. Both are readable and authoritative accounts. Both articles assume a minimum of specialized knowledge, building up the major developments in the fields in question from fundamental principles and indicating clearly the unsolved problems and possible lines of future development.

The two longest articles in the book are "The Biophysical Aspects of Nervous Function" by B. D. Wyke and "Biological Actions of Ionizing Radiations" by L. H. Gray. These are detailed discussions of extremely complex phenomena, and thus the topics are not developed in such bold outlines as in the first two articles discussed above. Nevertheless, they can be read with pleasure and profit by all from the beginning student in the field to the expert, although the specialist will derive the greater benefit.

R. W. G. Wyckoff's "Electron Microscopy of Developing Bacteriophage, and other Viruses" discusses the formation of viruses in living tissue as deduced from electron microscopy of thinly sectioned tissues. The article is illustrated by some of the magnificent electron micrographs for which Dr. Wyckoff is famous. "Physical Principles Underlying the Clinical Use of Radioactive Isotopes" by W. K. Sinclair and L. F. Lamerton actually concerns itself primarily with the techniques used. The rapidity with which this particular branch of medical physics is growing is evident from the development, in the short time since the article was actually written, of some of the methods here described. "Birefringence of Cytoplasm and Cell Membranes" by M. M. Swann and J. M. Mitchison, and "Physicochemical Properties of the Nucleic Acids" by D. O. Jordan presume a fair knowledge of the fields, and serve as valuable summaries of work published in the last few years.

All in all, the book is a valuable one, and can be recommended strongly to anyone wanting to keep abreast of modern activities in biophysics.

FRANKLIN HUTCHINSON

GENERAL BIOLOGY. 4th ed. By James Watt Mavor. New York, the Macmillan Company, 1952. 875 pp. $5.75.

This edition differs from the third in that the sections providing a background of chemistry have been removed from the text to an appendix, where they appear with a classification of living things and a glossary. Also, a few chapters have been largely re-written.

It is interesting to compare this edition with the first, with which the reviewer was acquainted (perhaps insufficiently) as an undergraduate. There is a noticeable addition of material of a biochemical nature, and in the physiological sections a slight shift towards explanations in terms of chemical systems. The reviewer appreciates the difficulty of explaining biochemical reactions to those utterly unfamiliar with chemistry and physics, but still feels that this aspect of the book is the weakest. For example, it would not seem too advanced to explain that the direct action of light in photosynthesis is to split water. Also, in speaking of urea formation it is misleading to represent the process as a reaction of ammonia with water and carbonic acid.

Other aspects, particularly the morphologic and genetic, are straightforward and are arranged in such a way that students are able to find information easily. (The reviewer was not pleased to find the diatoms classified as green algae.)

A most pleasant feature of the book is its scope, from rudimentary biochemistry to paleontology and the geologic history of man—a range which ought somewhere to overlap the interest of any student.

As far as style is concerned, Mavor's *General Biology* has not the liveliness and enthusiasm of Pauli's *The World of Life,* and certainly not the fine prose of a Borradaile and Potts (*The Invertebrata*). Sometimes the sentences are choppy and short, which may result from an effort to make statements easy to grasp. One feels that a little polish, even at the cost of some simplicity, would be a definite improvement and would increase for its owner the long-term value of this fine text.

R. R. L. G.

POLIOMYELITIS PAPERS AND DISCUSSIONS PRESENTED AT THE SECOND INTERNATIONAL POLIOMYELITIS CONFERENCE. Philadelphia, J. P. Lippincott Co., 1952. 543 pp. $7.50.

This 555-page volume which contains the Proceedings of the Second International Poliomyelitis Conference held in Copenhagen in September, 1951, is, to say the least, an imposing publication. It outdoes the high standard set by an earlier volume recording the events of the First International Conference held in 1948. Indeed, few publications dealing with the proceedings of a medical meeting of this type have appeared which can be compared with it as far as format, printing, and illustrations are concerned.

The book describes a program which was divided into four Symposia and three Reviews. The former deal largely with what might be called "biological aspects" of poliomyelitis, including the general subject of virology; the Coxsackie group of viruses; and differential diagnostic laboratory tests. The latter or the Reviews, deal with the pathogenesis, patho-physiology, certain aspects of diagnosis and treatment of poliomyelitis with emphasis on the difficult cases, and the after-care, and epidemiology.

There are also 150 pages devoted to the Scientific Exhibits and most of these lend themselves very well to this type of publication in which the editor has been so generous in allowing many pictures and even colored plates.

Difficult as it is to summarize the contents of a variegated program of this kind, it would be a mistake not to indicate that this Conference was marked by one great discovery which has already placed the clinical, epidemiological, and experimental aspects of poliomyelitis on a very different level from that on which it stood in 1948. I refer to the work of Dr. Enders and his group at the Children's Hospital in Boston on the growth of poliomyelitis viruses in tissue cultures. This, as was pointed out in a summary of the events of the Conference, represented a milestone marking the end of one era in poliomyelitis research, namely, the end of "the monkey era."

Other important subjects, many of them new, which were reported at this Conference were: the initiation of the first of a series of serological diagnostic tests which eventually may find wide clinical application; the report of the program on the typing of strains of poliomyelitis virus, which was initiated by the National Foundation for Infantile Paralysis and which has led to a great increase in knowledge regarding the circumstances under which the three different types of poliomyelitis virus occur; and again the introduction of antibody surveys in different populations which have indicated how different the ages may be at which people acquire immunity to poliomyelitis in different communities and the possible epidemiological signifi-

cance of this. Another feature of considerable clinical importance concerns the possible unfavorable effect which traumatic stress and certain inoculations may exert on the early stages of acute disease. All of these and other subjects received wide discussion.

Readers of this volume will do well to read carefully the introductory address entitled, "Medical Research and Natural Philosophy" by the great Danish scientist, Niels Bohr, whose remarks can be said to apply to various types of scientific work in general and to the reports in this volume in particular. They remind us that scientific results can only be received in the light of the circumstances under which the results have been obtained, and that, however important and final they may appear as recorded in this or any other similar volume, actually they are only current reports recounting what the up-to-the-minute story is, at this particular time.

JOHN R. PAUL

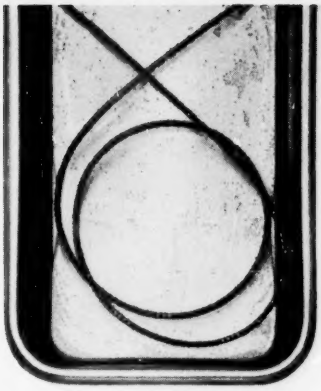
ATLAS OF MEDICAL MYCOLOGY. By Emma Sadler Moss and Albert Louis McQuown. Baltimore, The Williams and Wilkins Co., 1953. 245 pp. $8.00.

Any mycologist would be pleased indeed with the abundance of excellent plates which so enrich this text, for they constitute a wealth of gross and microscopic pathologic specimens as well as detailed colonial and morphologic studies of all the pathogenic fungi. While the photographs will benefit the student of mycology as a pictorial review of the pathogenesis of these microörganisms, the volume was written primarily to provide for the clinician a ready reference and basic procedure in elaborating the etiologic agent of a specific fungus infection.

To recall some basic concepts of mycology, the identification and orientation of medical pathogens among the Thallophyta or rudimentary plants are lightly sketched and supplemented by glossary. Against this brief background, which would not be adequate for the neophyte, each fungus of clinical significance is described in relation to the symptoms of the disease followed by the specific procedures for the isolation and identification of the fungus, the mycologic data that can be derived from histologic studies of biopsy and necropsy specimens, and concluding statements concerning the therapy of choice as well as prognosis. Although the volume is replete with formulary, a culture media section, and chapter on bothersome contaminants, the methods described imply a knowledge of techniques familiar to the dermatologist and mycologist, but which may not be in the armamentarium of the average physician. From the viewpoint of stark practicality, therefore, the book is found wanting, but for visualization of the clinical and histologic aspects of fungal disease the "Atlas" is beyond competition.

Despite the realization that black and white photographic plates in lieu of drawings in color are the price of economy, it must not be forgotten that many photographs fail to convey the specific differentiating details intended and are worthless. This serious criticism can be made of several plates demonstrating colonial morphology. If the fungus species cannot be identified from such photographs by the mycologist—and they cannot—the plates will mean nothing to the uninstructed. It would seem that by deleting these few plates and providing the minor additions indicated, the authors would have augmented their great service and extended the usefulness of a truly valuable book.

M.-L. J.



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1. Canad. M. A. J. 66:151 (Feb.) 1952.
2. J. Urol. 67:762 (May) 1952.
3. Ibid. 69:313 (Feb.) 1953.

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